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THE EFFECTS OF TENSION AND QUALITY OF THE METAL UPON THE CHANGES IN LENGTH PRODUCED IN IRON WIRES BY MAGNETIZATION.

By Byron B. Brackett.

Historical.

FIFTY years ago a machinist of Manchester imagined that he could see the volume of a mass of iron increase when it was magnetized and decrease when the magnetizing force was removed. Hoping to be able to use this principle in the construction of an electro-magnetic engine, he appealed to Dr. Joule[1] to investigate the phenomenon and determine the amount of the change. By immersing the iron to be magnetized in a closed vessel, filled with water, in which stood a fine capillary tube Joule could not detect any change of volume, though it has since been shown[2] that had he used either stronger or weaker fields he probably would have done so. But by a system of compound levers of great multiplying power he proved that an iron bar did change it length when magnetized longitudinally. He observed an increase in length of 1–200,000. As a result of his investigations he proposed the following laws:

1. When soft iron rods are magnetized their length increases

[1] Joule, Phil. Mag., (3), vol. 30, pp. 76, 225.
[2] Bidwell, Proc. Roy. Soc., vol. 56, p. 94.

and the elongation is approximately proportional to the square of the magnetizing force.

2. Tension applied to the rod diminishes the elongating effect.

3. The elongation is greater for the same intensity of magnetism in proportion to the softness of the metal.

That the two first laws are correct for the fields that Joule used cannot be doubted; but as to the third law there is much uncertainty. The investigations of Shelford Bidwell[1] indicate that not only hardening, but also annealing an iron rod diminishes the elongating effect, and at the best the relation between the softness of the iron and its change of length is to-day very much confused.

It was nearly twenty-five years after Joule's investigations before the question was taken up again experimentally by Barrett[2] and nearly at the same time by Mayer.[3] Barrett employed the tilting mirror, which is described under the apparatus used in this investigation, a device suggested to Barrett by Professor Rowland. He experimented not only upon iron, but also upon nickel and cobalt.[4] He observed an elongation of 1–260,000 for iron and 1–425,000 for cobalt; and a contraction of 1–130,000 for nickel.

Mayer found an elongation of 1–277,000 for iron. Some of his observations upon the action of hard steel seemed to be at variance with Joule's results. But Bidwell has since shown that this apparent difference was due solely to their different methods of experimenting.

In 1885 Bidwell[5] reported the first of a very extensive series of experiments upon the distortions caused by magnetization in iron, nickel and cobalt. He carried his investigations up to fields many times stronger than those used by the earlier investigators. He has worked upon the effects of tension, tempering and annealing. He has experimented with both rods and rings. He found that, at least with his apparatus, rods did not continue to elongate, but reached a minimum length, then gradually shortened until they had less than

[1] Bidwell, Proc. Roy. Soc., vol. 55, p. 228.

[2] Barrett, Phil. Mag., 1874, vol. 47, p. 51.

[3] Mayer, Phil. Mag., 1873, vol. 45, p. 350; Mayer, Phil. Mag., 1873, vol. 46, p. 177.

[4] Barrett, Nature, 1882, vol. 26, p. 585.

[5] Bidwell, Proc. Roy. Soc., 1885, vol. 38, p. 265; 1886, vol. 40, pp. 109, 257; 1888, vol. 43, p. 407; 1890, vol. 47, p. 469; 1892, vol. 51, p. 495; 1894, vol. 55, p. 228; 1894, vol. 56, p. 94; Phil. Trans. Roy. Soc., 1888, vol. 179 (A), p. 205.

their initial length, apparently approaching a limiting value asymptotically. Investigations upon this subject have also been made by Berget,[1] Bock,[2] Jones,[3] Knott,[4] Lochner,[5] Nagaoka ;[6] and two years ago investigations in this line were begun here, in the Johns Hopkins laboratory, by Dr. L. T. More.[7]

At Professor Rowland's suggestion, Dr. More determined the intensity of magnetization in his wires for each change of length observed, and also sought to take into consideration the secondary actions that might affect the length of the wire.

Professor Rowland defends his position on the subject as follows :

The change in length may be partly due to other causes than the magnetization of the metal. Among these one can put the stresses due to magnetization. It is not at all evident that these stresses can be exactly identified with the Maxwellian stresses. If we think of the long wire that More used as composed of a bundle of small elementary magnets tied together at points well inside the poles, the magnets would seem to have no tendency in their central parts to separate from each other ; and in that case there would be no pressure at right angles to the lines of induction, unless it can be shown to result from a squeezing outwards of some kind of matter caused by a longitudinal compression.

At the same time, the compressive force which, it is known, will tend to close up a very thin air-gap in a divided magnet must also exist in any magnet, for, according to all our ideas of matter, there is no real difference in the case where the air-gap exists and where it does not ; because we still must consider the gaps between the molecules. If we now think of the long elementary magnets as composed of short elementary pieces with their ends so near together that the effects of their poles are neutralized in all action

[1] Berget, Comp. Rend., tom. 65, p. 722.

[2] Bock, Wied. Ann., 1895, vol. 54, p. 442.

[3] Jones, Phil. Mag., 1895, vol. 39, p. 254.

[4] Knot, Phil. Mag., 1894, vol. 37, p. 141 ; Proc. Roy. Soc. Edinburg, vol. 18, p. 315 ; vol. 20, p. 290 ; vol. 20, p. 334.

[5] Lochner, Phil. Mag., 1893, vol. 36, p. 504.

[6] Nagaoka, Wied. Ann., 53, pp. 481, 487 ; Phil. Mag., 1894, vol. 37, p. 131 ; 1896, vol. 41, p. 454.

[7] More, Phil. Mag., 1895, vol. 40, p. 345 ; Phys. Rev., 1895, vol. 3, p. 210.

upon external bodies and yet allow a space between their ends for a compressible medium to entirely surround them, then this longitudinal pressure would cause both longitudinal shortening and a pressure outwards perpendicular to the induction. It would seem that such a case might represent both the strain in the medium and the strain in the ether.

The value of this compressive force is probably $\frac{B^2}{8\pi}$. It may possibly be $\frac{(B-H)^2}{8\pi}$, but in most cases the two are so nearly equal in value that it would not seriously alter the results to take either force. Taking this as equivalent in its action to a simple mechanical pressure and considering, with it, the ordinary elasticity of the wire, the shortening due to this cause can be computed for each observation and corrections may be made accordingly. Or this force might be considered to neutralize a portion of the tension on the wire equal to it in value.

For reasons like these, Professor Rowland advised Dr. More to correct his observed readings of elongation for a shortening caused by the force $\frac{B^2}{8\pi}$; and these corrected readings were plotted to a basis of induction in the iron instead of being given on a basis of the magnetizing force, as all previous curves in this subject had been given, except those of Nagaoka.

Last year Dr. E. F. Gallaudet[1] made an investigation on this subject in this laboratory. He used the apparatus employed by Dr. More, and his curves were in all respects except one similar to those obtained by Dr. More, Bidwell and others. He did not correct them for a contraction caused by a force of $\frac{B^2}{8\pi}$.

An initial contraction was observed by him that no one has announced before, and he also gave observations that seemed to indicate a very great change in the values of Young's modulus as the magnetization increased. That there could be no real change in the elasticity so large as these, can be shown from Dr. Gallaudet's own tables; for such changes in value of Young's modulus under

[1] Gallaudet, J. H. U. Thesis, June, 1896.

the larger tensions used would have caused changes of length in the wire many times greater than the changes observed. Moreover, Bock[1] found as a result of his experiments that the changes in elasticity due to magnetization could not exceed one-half per cent.; and Miss Noyes,[2] in a series of experiments upon the changes of Young's modulus with temperature, did not find any evidence of a change of the elasticity from magnetization.

Object of this Investigation.

It was hoped that by carefully observing the behavior of quite a number of different wires under varied physical conditions data might be obtained that would explain the relation of the elasticity of the metal to its change in length under magnetization. It was also hoped that incidentally the question of an initial contraction preceding elongation, as observed by Dr. Gallaudet, might be settled; and that likewise some additional information on the relation of the temper of the metal to its change of length might be obtained.

Apparatus.

I employed the apparatus used by Dr. More and by Dr. Gallaudet. It is very fully described by Dr. More, who[3] gives all the dimensions essential to its construction. The feature that is essentially characteristic of this apparatus, in addition to Professor Rowland's tilting mirror that both Barrett and Bidwell used, is the cylindrical jacket which was suggested by Dr. Ames. The figure here given will show its use. About the part of the wire where the changes in length are to be observed is placed a brass tube having a free internal diameter of over one centimeter. At the bottom end *b* the tube is clamped firmly to the wire while the upper end has a loosely fitting cork merely to keep the axis of the cylinder and the wire coincident. To the top of the cylinder is attached a projecting arm that carries two raised supports *a* and *c*. Above this arm is placed a lever *d*, resting by a knife-edge on the support *a*. This lever also has an inverted knife-edge but a very short

[1] Bock, *loc. cit.*

[2] Mary C. Noyes, Phys. Rev., 1896, vol. 3, p. 432.

[3] More, *loc. cit.*

distance back of the one that supports its weight, and over the in-inverted knife-edge is placed a hook that is firmly clasped to the wire at *g*. Thus any change in the length of the wire between the

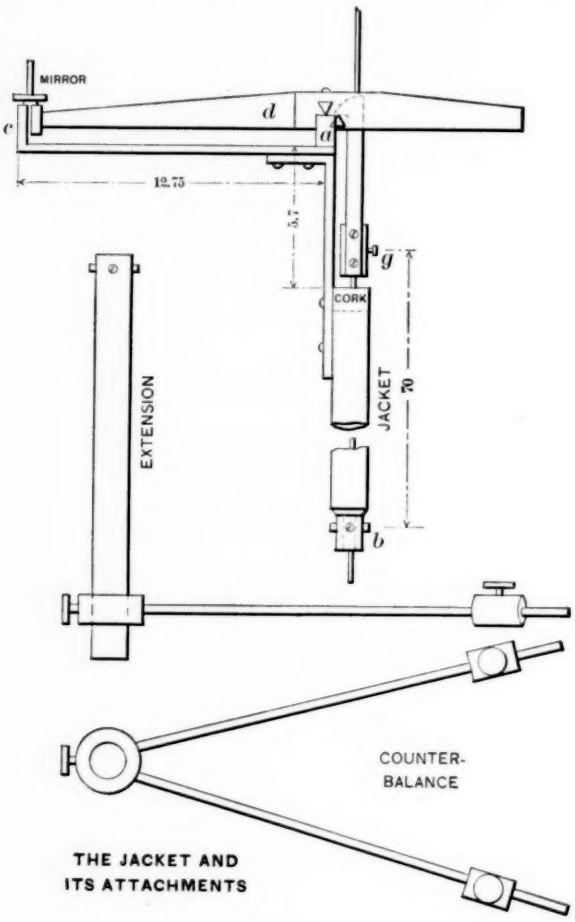

Fig. 1.

points *b* and *g* will cause the outer end of the lever to rise or fall. Standing partly on the end of this lever and partly on the support *c* is a little table having three needle-point legs and carrying a vertical plane mirror. Now any movement of the lever arm relative to the support *c* will cause the mirror to tilt; and by observing with a telescope at some distance the image in the mirror of a ver-

tical scale the movement of the lever arm will be greatly multiplied.

If,

L = length of the long arm of the lever,

l = length of the short arm of the lever,

d = distance from one leg of the brass table to the line joining the other two legs, resting on lever end,

D = scale distance from the mirror, then the multiplying power of the apparatus is

$$\frac{L}{l} \times \frac{D \times 2}{a}.$$

$L = 11.672$

$l = 0.4776$

$d = 0.3335$.

D differed slightly for different tests, but was nearly always as great as 170. Thus the multiplying power of the apparatus was always approximately 25,000.

To determine the elongation caused by the tensions used in most of the tests, and to check the values of the modulus of elasticity determined from adding small weights, the mirror at c was removed, and another mirror was attached to the lever d just above the point a, so that the apparatus acted as a simple multiplying lever. This arrangement gave a multiplying power of only $2\frac{D}{l}$, or about 700, and so was capable of being used for much greater elongations.

The wire to be tested is always suspended from a point some distance above the apparatus in such a way that the jacket has its ends well within the magnetizing solenoid. The solenoid rests upon a support entirely independent from that of the wire and those parts of the apparatus attached to the wire; and the two parts are so adjusted that there is no contact between them.

I made but two changes in the apparatus itself that could be supposed to affect the results. In the apparatus as used by Dr. More and by Dr. Gallaudet, the jacket and its attachments were not perfectly balanced. The lever d was as nearly balanced as was desirable to have it. But all the weight of the lever came on one side

of the wire; and that together with the weight of the arm under it, gave quite a tendency to the jacket to tip sidewise and slightly bend the wire. Then the amount of this bending would doubtless be changed by the magnetic stresses; and especially when no weight was attached to the lower end of the wire, it seemed that this might possibly have quite an effect upon changes so small as those to be observed. An adjustable extension was therefore made to attach to the lower ends of the jacket, extend well down below the magnetizing solenoid, and by means of sliding weights on its two arms exactly counterbalance the moment given the jacket by the arm at the top, together with the lever and mirror. Again, the jacket had been clamped, at its bottom, to the wire by a single set-screw. This it seemed might cause a bend in the wire. The brass plug *b* was therefore bored out quite a little larger than the wire. Two extra radial set-screws were put into *b*, making three equally spaced ones about the circumference. Then a little cylinder of brass, slit lengthwise on one side, bored along its axis to fit the wire and just large enough to slip into the hole in *b*, was put about the wire at the point of attachment. In this way by the use of the three screws the wire could be accurately centred in the jacket.

These precautions seemed necessary, for a slight, almost imperceptible bend in the wire was found by actual experiment to greatly modify the readings obtained. These adjustments were so delicate that it was scarcely possible to judge when they had been properly made except by actual trial. After Young's modulus had been carefully determined, it was considered a fair test of the adjustment of the apparatus to put on and off a weight that ought to cause changes in length approximately equal to those expected to follow from magnetization. If the computed readings were obtained, the adjustment was assumed to be correct.

A most evident source of error in this work is the change of length caused by temperature. The greatest value of $\frac{dl}{l}$ observed in my investigation which seemed to be due to magnetization was 33.92×10^{-7}. A change of one degree centigrade in the temperature of the wire would give for $\frac{dl}{l}$ about $120. \times 10^{-7}$. To pre-

vent temperature changes the magnetizing solenoid was made originally from two coaxial brass cylinders in such a way as to leave a space for water between them. In all the experiments of this investigation water was being continually forced into this space from the bottom and was overflowing at the top. Thus there was a jacket of continuously changing water between the magnetizing coils and the jacket attached to the wire. Yet if the larger currents were left on even for a short time the effects of temperature changes were quickly visible. If the conditions have become steady and all the apparatus has attained a constant temperature, and then a current is put on and allowed to remain, the resulting higher temperature first affects and expands the jacket on the wire, causing an apparent *contraction* of the wire itself, and as the higher temperature reaches the wire *expansion* will be observed until at last, in the steady state resulting, the wire will have apparently expanded or contracted according to the relative coefficients of heat expansion for the wire and for the jacket. Fortunately, these changes are comparatively slow and can be separated with a limited degree of accuracy from readings that can be taken quickly. But if one were to take a series of readings from the modulus of elasticity, using small changes of weight while the magnetizing current remained on, it might be foreseen that the results would apparently vary first in one direction and then in the other, due to the unequal changes in the length of the jacket and the wire caused by the heating.

Incident to the great multiplying power of the apparatus, the slightest mechanical vibration of the wire under test made it absolutely impossible to read the changes of length accurately, and, in many cases, to read the figures of the scale at all. To get a support for the wire as free from vibrations as possible, a weight of some seventy-five or eighty pounds was made by filling a wooden box with bricks and old storage battery plates, and this was suspended by a single coiled brass spring to an arm projecting from a side-wall of the room some ten feet above the apparatus. The spring was so designed that, with the weight it carried, its period of vibration was very slow, and when the wire to be tested was suspended from the bottom of the weight very little trace of the ordinary vibrations of the building ever reached the wire. But to have the

system work perfectly, it was necessary that the wire and the weight which it carried be attached exactly under the center of mass of the large weight above. To facilitate this adjustment four projecting arms were attached to the bottom of the box forming the weight and on these arms were hung small movable baskets of shot. When all was properly adjusted the scale image in the telescope would be blurred for a half-minute by a wagon passing on the cobble-stone pavement some twenty or thirty feet from the apparatus, but otherwise all was undisturbed.

General Method of Working.

It was decided that, for each kind of iron tested, series of elongation readings should be taken for several different tensions, that the elongations should be observed at each point both with the magnetizing field on and with the field off, and that for all observations of elongation the corresponding inductions in the iron should be obtained.

When the wire had been properly adjusted in the apparatus, with a weight attached to the lower end of the wire to give the desired tension and after the test with the small weight already mentioned had been made, the first elongation reading was taken by sending a very weak current through the magnetizing solenoid, quickly observing the scale reading in the telescope and then breaking the current and reading the telescope again. This procedure was repeated with gradually increasing values of the current until the maximum current was reached. The elongation readings obtained with the field on, I shall call the "total" elongation, since it represents both the temporary and the permanent or residual change in length. The elongation observed after a certain magnetizing field has been put on and then taken off, will be called the "residual" elongation for that definite field.

All the currents used in magnetizing were left on only long enough to get the required readings accurately. A very careful watch was kept for temperature changes and when they began to appear, the readings were stopped until all had cooled down to the steady state again. The currents used were measured by carefully tested Weston instruments of different capacity according to the currents to be read.[1]

[1] See end of article for notation used in tables and plates.

The elongation data given to this investigation were all taken the first time the wire was magnetized after it had been put into the apparatus. No method seemed efficient in completely demagnetizing the wire while it was in place in the apparatus. Alternating currents would not do it, probably because the ends of the wire extended so far beyond the solenoid. After using the alternating current, the ends of the wire would still show a decided polarity of the same kind that it showed before, and the effects upon the elongation curve were very marked. If the magnetizing field was applied again in the same direction as before the curve was very similar to the one obtained at first though the amount of the elongation was somewhat reduced. But if the field was applied in the opposite direction the first effect with weak fields was a contraction, followed by a decrease of the contraction, then by elongation and the rest of the curve was like other curves for the same wire.

The inductions corresponding to the elongations were obtained from a separate series of readings by means of the ballistic galvanometer, using the method of increasing reversals and breaking the current to get the difference between the total and residual inductions. It was exactly the method used by Professor Rowland[1] in his well known ring experiments. Though actual test had shown the field to be practically uniform along that part of the solenoid where the elongation of the wire was measured, yet, as a precaution against irregularities in the induction of the wire, the test coil was distributed in four equal parts equally spaced along the portion of the wire under observation. For the lower inductions seven hundred turns were used, while for the higher inductions this number was reduced to four hundred turns. The galvanometer with its circuit was calibrated for each connection; and, moreover, the ratio of the deflections obtained by the two different arrangements was always taken each time the change was made.

With soft iron wires the alternate current was applied after the elongation readings had been taken and the induction readings were then made from the same wire. While the elongation curve would have been quite different under the second magnetization, yet actual test showed that the first induction curve taken for any wire did not

[1] Rowland, Phil. Mag. (4), vol. 46, p. 140.

differ from subsequent ones. For the hard wires tested the induction curves were taken from different wires apparently exactly like the ones used for the elongation tests.

WORK DONE.

During my investigation I made not less than thirty-five elongation tests. All of the more important series of readings were repeated, with different wires, several times, in order that none of the curves should represent chance results. No series given in the tables rests upon the evidence of a single experiment. Tests are reported upon piano-wire in its natural condition under two different stresses, annealed piano-wire under three different stresses and soft annealed iron wire under four different stresses, making nine series in all.

The lower tension used for natural piano wire was the smallest that would apparently free the wire from bends, for it could not be obtained in a perfectly straight form. The larger tension was a little more than half way to the elastic limit. These wires have a diameter of 1.25 mm. The same wires used in these tests were then heated to a bright red by passing a current through them and were allowed to cool slowly in the air. The slightly burned outside of the wires was then carefully removed by the use of fine flint-paper, after which they were tested under the same tension as before. Since the wires came out of the annealing process perfectly straight it was possible with these wires to make a test under the tension caused by the apparatus alone.

But neither with the natural nor annealed piano wire were the changes caused by magnetization very great, or the effects of a permanent strain especially marked. The most extensive investigation was therefore made upon very soft annealed iron wire. The different pieces were cut from one continuous piece, and parts taken out at different places were analyzed by Mr. Nakaseko of this university, and were pronounced by him to be very pure and quite free from carbon as well as from all other impurities. All of these wires were heated to the bright red state just as in the case of the annealed piano wire, but here it was only to free them from kinks. They gave the same magnetization curve before and after this process; and

while the same elongation curve could not be obtained in the two cases, all of my work would lead me to think that the difference was due solely to the bends in the wire before the heating process.

This wire as tested had a diameter at 1.31 mm. Its behavior was investigated under five different tensions, ranging from that due to the apparatus only up to the elastic limit of the wire. Under the largest tension the wire would elongate the usual amount upon the addition of a very small weight, but would require quite a little time to recover when this weight was removed. Possibly because the elastic changes were so slow under this tension, it was impossible to take both the total and residual elongation curves. The best that could be done was to keep the current on all the time and correct as well as could be done for the temperature changes to obtain the curve for total elongation.

Not only were more tests made on this kind of iron, but the readings obtained have been worked out, somewhat more fully. Elongation curves from these results have been plotted both to H and to I, and in both cases the contractions that Professor Rowland believes will be caused by the force $\frac{B^2}{8\pi}$ have been plotted in a separate curve and the elongation curve as modified by this contraction has also been given. For the residual curves this supposed contraction is computed from $\frac{(4\pi I)^2}{8\pi}$, the residual value of I, of course, being taken.

The value of Young's modulus used in computing the contraction curves for this iron was 2.12, and was determined both from small changes of tension, using the higher multiplying power and from larger changes of tension using the lower multiplying power. The latter method was considered the more reliable, especially in determining the elongations due to large tensions. But this method could not be used to investigate the possibility of a change in modulus caused by magnetization, since it required so much time to adjust the weights that temperature effects from the magnetizing current would surely modify the results. By using the smaller changes of tension, no variation in the modulus could be detected at any stage of magnetization, except such as were within the limits of error of the method.

It was desired to take the induction readings for exactly the same values of the magnetizing current that had been used in the elongation tests; but on account of the bad condition of the storage batteries that I had to use during most of my work, I could only make the currents approximately equal in the two cases. The points on each curve, especially where there could be any doubt as to the exact location of that curve, were taken very close together; and then through interpolation on the curves, the corresponding values of elongation and induction could be found with as great accuracy as the curves could be plotted.

GENERAL RESULTS.

Speaking very generally, all the curves of elongation when plotted to H show either no change of length up to nearly the field at which the magnetic saturation occurs or up to that point their elongation curve is similar in shape to the induction curve. Not far beyond this the residual curves become practically horizontal and show no further change in length of any consequence; while the curve of total elongation begins at nearly the same point to descend along an approximately straight line. The residual curves for soft iron seem to fall quite a little after the higher fields have been applied. But while there seems to be no doubt that a slight shortening of the wire occurs, due probably to a rearrangement of the molecules under the strain of the higher fields, yet, I am sure that this shortening is exaggerated in my readings by temperature effects that could not be avoided. All the changes of length due to any change in the magnetic conditions are very quick; and the almost instantaneous throw of the lever and mirror from one position to another, when quite large, as it was here, caused vibrations that required a little extra time to die out, and consequently allowed some heating of the jacket to occur. All attempts to damp this vibration involved the possibility of displacing parts that must move so freely as the lever and the mirror; and to put the current on and off gradually involved about the same possibility of heating as did the time required for the vibrations to cease. It thus seemed best to make these readings as well as could be done and then give them with the above caution. In some cases I have drawn the ends of

the curves somewhat above the plotted points, thinking them much more nearly correct there than if drawn through the points. For the same reason I think it probable that the ends of the total elongation curves should show a marked tendency to curve upwards from the straight line, as has been found by Bidwell.[1]

Effect of the Field.

In any case the residual curve shows that for quite a distance beyond the point of saturation no permanent change of length occurs; and the curve of total elongation shows, through these same field values, a contraction directly proportional to the field strength. Thus these two curves, at least along this portion, seem to show clearly that there is an elongation due directly to the induction in the iron and a contraction caused by the field, directly proportional in value to the field strength, as has been suggested by Dr. Gallaudet.[2] Every set of curves given in this investigation seems to show a contraction due to the magnetizing field. Where plotted to I, the elongation, in any one test, is always greater for the same value of induction when the field is off than when it is on; and the curves modified by the supposed contraction due to $\frac{B^2}{8\pi}$ and $\frac{(4\pi I)^2}{8\pi}$, both when plotted to H, and when plotted to I, indicate the same thing.

Increased tension on the wire apparently causes the contraction to begin at a lower field and at a lower induction in the iron; but when the elongation reaches the straight line portion of the total elongation curve plotted to H, it continues on the same slope without any regard to the tension upon the wire. In other words the straight portions of all these curves for the same kind of wire are parallel. For another kind of wire the slope of this portion of the curve will be entirely different; but here again the curves for this wire, taken under different tensions, will have parallel parts. This would seem to indicate that the contraction caused by the field is not only proportional to the field, but that it depends also upon some definite constant for each kind of iron. For the three different kinds of iron that I have studied these constants would have to be about in the ratio of 12.5, 18 and 29 for the natural piano wire, an-

[1] Bidwell, *loc. cit.* [2] Gallaudet, *loc. cit.*

nealed piano wire, and the soft iron respectively. It does not seem possible to identify these values directly with either the elasticity or the magnetic permeability of the wires. The curves as modified by the $\frac{B^2}{8\pi}$ correction would have a slightly smaller absolute value for these constants, but the ratio of their values would be very nearly the same.

Looking at the total elongation curves plotted to I, it will be seen that the retraction of length does not appear until the value of μ becomes very much reduced, or until a relatively large increase of the field is necessary for a small increase of induction in the wire. As the field necessary to give a certain increase of induction becomes larger and larger, the curves show a more and more rapid contraction. Since this part of the curve gradually becomes more and more nearly vertical it seems evident that it must approach asymptotically the vertical line drawn through the maximum or limiting value of I, which for this soft iron would doubtless be about 1700.[1] This would indicate that there is no limit to the contraction which is caused by the field, but the contractions resulting from a definite amount of field increase might finally become less and less, as Bidwell[2] has observed.

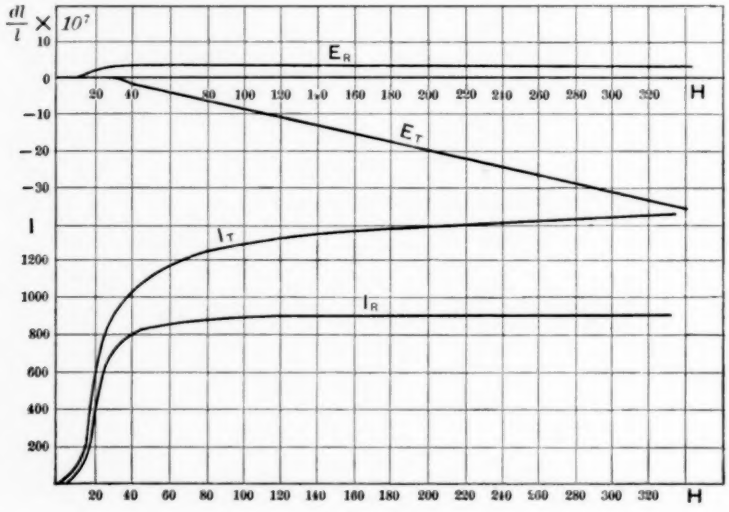

Plate 1.

Natural Piano Wire: Tension 658. (See Tables 1 and 2.)

[1] Ewing, Magnetic Ind., p. 145. [2] Bidwell, *loc. cit.*

TABLE I.

Elongation data for piano wire in natural state and under tension of 658 *kg. per square cm.*

$\frac{dl}{l} \times 10^7$ due to this tension = 2933.

H	E_T	E_R	H	E_T	E_R
.914	.00	..00	59.49	−3.92	3.36
1.83	.00	.00	69.50	−5.04	3.36
2.74	.00	.00	79.50	−6.16	3.36
4.11	.00	.00	94.15	− 7.28	3.36
5.49	.00	.00	104.2	− 8.67	3.36
8.23	.00	.00	116.1	−10.09	3.36
9.14	.00	.00	132.7	−11.78	3.36
13.09	.00	.56	143.1	−13.15	3 36
15.90	.00	1.12	155.5	−15.12	3.36
19.68	.00	1.68	167.3	−16.24	3.36
22.41	.00	2.24	183.0	−17.92	3.36
26.85	.00	2.80	203.5	−20.73	3.36
29.95	−0.56	3.36	219.6	−21.85	3.36
33.15	−0.84	3.36	240.0	−24.66	3.36
38.10	−1.40	3.36	260.6	−26.90	3.36
45.08	−2.52	3.36	278.8	−29.68	3.36
48.72	−3.08	3.36	301.8	−31.39	3.36
53.50	−3.47	3.36	338.5	−35.30	3.36

EFFECTS OF TENSION.

As to the effects of tension, it will be seen that up to the limit of my magnetizing field, the numerical sum of the elongation and contraction is nearly the same for all tests of the same kind of wire regardless of the tension used. But the greater the tension the less will be the elongation and the greater the contraction obtained. Whether we examine the curves as plotted to H or to I, we shall see that after the contraction begins, the length of the wire as compared to its length before magnetization, becomes less as the tension is greater. That is, if we disregard, for the moment, the elongations caused by the tensions used, and consider the wires to all have equal lengths when magnetization begins, then we may say that on the parts of the curves which we are considering, for equal fields or for equal inductions, the greater the tension the shorter the wire. For example, if we examine the total elongation curves, as

TABLE II.

Magnetization data for wire in Table I.

H	B	μ	I_T	I_R
1.83	98	53.6	7.7	.0
2.75	169	61.5	13.2	.75
6.40	459	71.7	36.1	6.4
8.09	707	87.4	55.6	14.1
10.75	982	91.4	77.3	25.1
12.71	1374	108.1	108.3	36.1
15.50	2394	154.6	189.2	99.2
19.45	5814	299.5	460.5	339.5
21.98	7747	353.0	615.2	488.0
26.65	10167	381.5	807.5	652.5
29.52	11020	374.0	874.5	709.0
32.82	11843	361.2	939.5	751.0
37.65	12578	334.0	999.9	794.0
40.73	13091	322.0	1038.	805.
44.45	13464	303.0	1069.	830.
48.00	13798	287.8	1095.	839.
52.75	14133	268.1	1120.	861.
58.35	14608	250.5	1159.	864.5
66.65	15117	227.0	1199.	876.
76.50	15637	204.5	1238	883.
91.42	16141	177.0	1238	890
101.2	16351	161.5	1294	896
112.5	16663	148.2	1318	898
128.4	16848	131.2	1331	900
139.2	16969	121.9	1338	900
151.0	17271	114.4	1363	902
165.0	17345	105.2	1368	902
178.2	17528	98.4	1381	904
198.3	17598	88.8	1385	904
217.4	17937	82.4	1412	906
238.0	18188	76.4	1428	908
256.2	18206	71.1	1428	908
274.5	18375	66.9	1440	910
297.3	18447	62.2	1445	910
331.8	18582	56.0	1453	913

plotted to H, for soft wire under least and under greatest tension we shall see that, after the curves begin to descend, all points on the curve of greatest tension are about 40 units below those of least tension. But before any change of length due to magnetization began the elongation caused by the tension, expressed in the

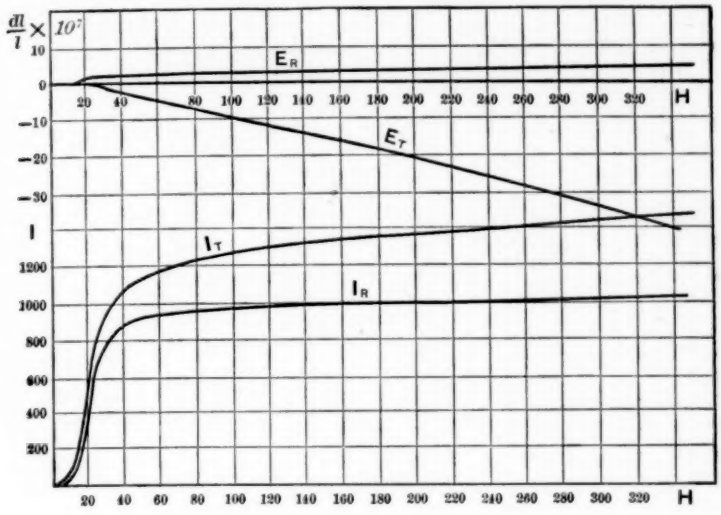

Plate 2.

Natural Piano Wire: Tension 1949.

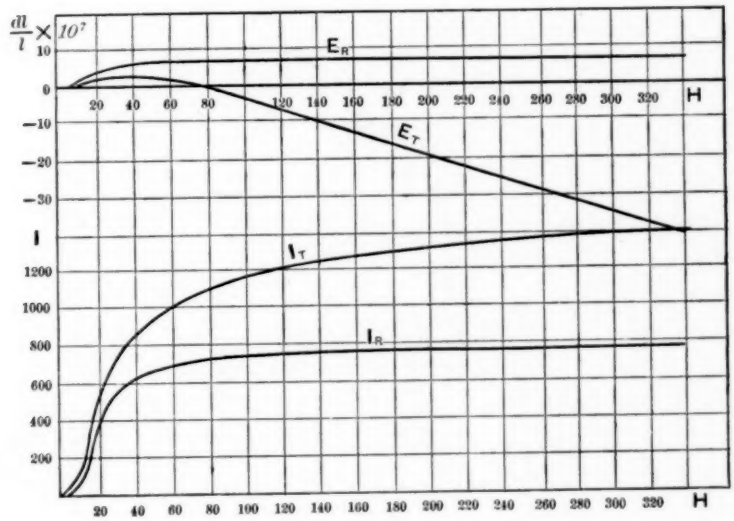

Plate 3.

Annealed Piano Wire: Tension 62.

TABLE VII.

Elongation data for annealed piano wire under tension of 699 *kg. per square cm.*

$\frac{dl}{l} \times 10^7$ due to this tension = 3178.

H	E_T	E_R	H	E_T	E_R
.914	.00	.00	52.18	−0.567	5.39
1.830	.00	.00	58.10	− 1.42	5.39
2.74	.00	.00	69.45	− 3.42	5.39
3.66	.00	.00	78.22	− 4.54	5.39
4.57	.00	.00	93.20	− 7.37	5.39
5.49	.00	.00	103.3	− 9.06	5.39
8.14	.00	.17	114.7	−10.78	5.39
9.23	.17	.567	131.2	−14.17	5.39
11.95	.567	.850	141.6	−15.88	5.39
12.81	.850	1.42	157.9	−18.70	5·39
15.54	1.70	2.27	166.4	−21.00	5.39
19.69	2.55	4.25	182.4	−23.25	5.39
21.99	2.84	4.82	203.2	−27.20	5.39
27.00	2.84	5.11	219.2	−30.61	5.39
29.72	2.27	5.11	237.9	−32.88	5.39
32.90	1.99	5.39	258.2	−36.84	5.39
37.92	1.70	5.39	274.2	−40.26	5.39
40.98	1.13	5.39	304.1	−44.22	5.39
44.38	.567	5.39	340.8	−49.02	5.39
48.00	.00	5.39			

same units used in the curves was 7950 in one case and only 223 in the other case. What probably occurs is a reduction of the original elongation of 7950 units by 40 units, and beyond this all the phenomenon is very nearly as in a case of no tension. Suppose, then, the wire to become less elastic with magnetization, and that the value of Young's modulus has increased by about one-half per cent.[1] when the turning point in the magnetization curve is reached; then the elongation that originally existed will be diminished by one-half per cent., and we have explained the greater contraction resulting from magnetization when a wire is under tension than when it has no tension. This change in the modulus of elasticity will explain the reduced elongations of all other curves in the soft iron series; and a similar explanation will apply to all curves

[1] Bock, *loc. cit.*

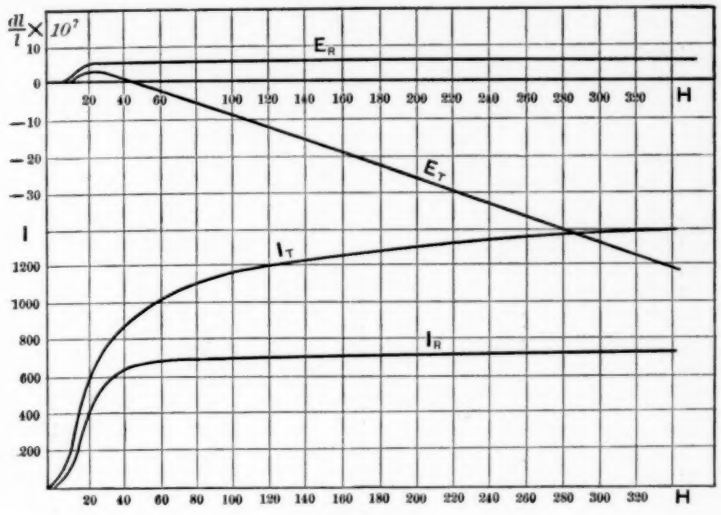

Plate 4.

Annealed Piano Wire: Tension 699. (See Tables 7 and 8.)

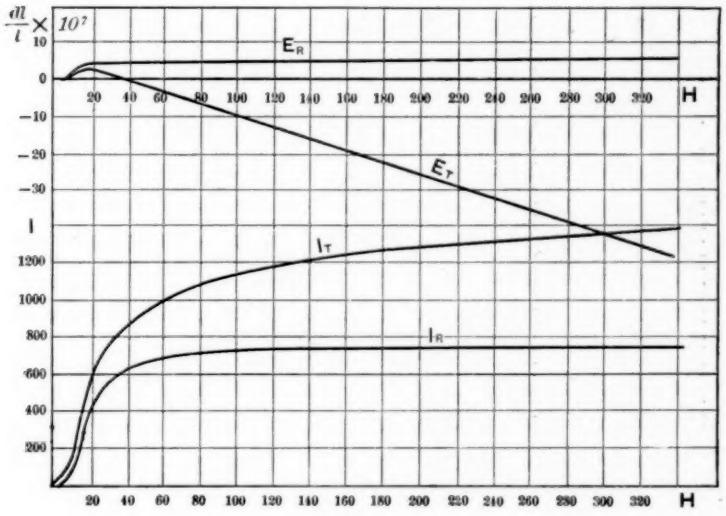

Plate 5.

Annealed Piano Wire: Tension 1949.

TABLE VIII.

Magnetization data for wire in Table VII.

	B	μ	I_T	I_R
1.83	99	55.	7.7	0
2.65	176	66.4	13.8	2.5
3.66	269	73.5	21.1	3.0
4.67	362	77.6	28.4	3.1
5.49	433	79.0	33.9	6.7
8.09	837	103.6	65.9	25.7
9.15	1024	112.0	80.8	27.7
10.74	1399	130.5	110.6	47.2
12.66	2077	164.5	164.2	88.3
15.18	3581	236.0	284.0	182.0
16.91	4352	258.0	345.0	227.0
19.19	5689	295.6	450.5	324.0
20.80	6306	303.8	500.0	361.8
22.41	6952	310.0	551.0	409.2
30.49	8990	294.0	715.5	553.5
36.80	10327	281.0	819.0	608.8
39.75	10740	271.0	852.5	629.0
43.25	11143	258.0	883.0	651.0
47.18	11577	245.5	917.5	656.0
51.45	11972	233.0	949.0	668.0
56.95	12407	218.3	984.0	679.0
69.01	13369	193.8	1060.	690.
77.00	13707	178.0	1085.	695.
91.45	14482	158.3	1142.	700.
101.6	14592	143.7	1152.	706
112.9	15043	133.0	1190	712
129.0	15389	119.2	1214	712
139.3	15629	112.3	1232	714
151.4	15871	104.6	1251	716
166.2	16066	96.4	1266	717
178.2	16428	92.2	1295	718
198.1	16558	83.6	1301	720
215.4	16915	78.5	1329	722
235.4	17055	72.6	1339	724
251.5	17282	68.7	1356	726
274.5	17375	63.2	1361	726
297.2	17497	58.9	1369	730
327.0	17917	54.8	1399	733

in this investigation, though the change of Young's modulus must be considerably less than one-half per cent. for the annealed piano wire and still less for the natural piano wire.

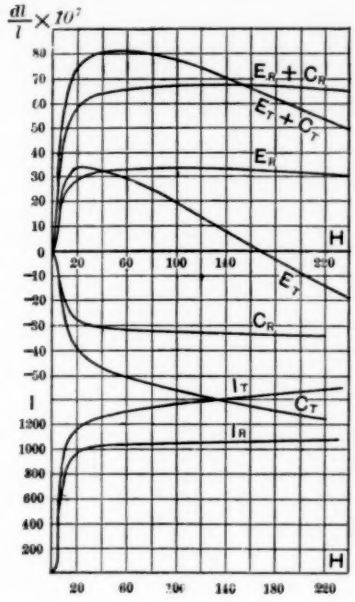

Plate 6.
Soft Iron: Tension 48.3.
(See Tables 11 and 12.)

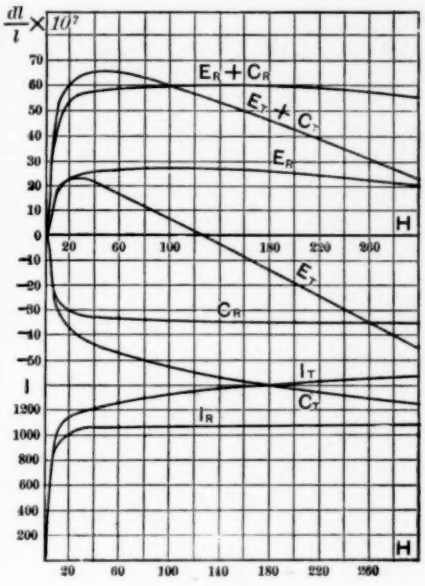

Plate 7.
Soft Iron: Tension 430. (See Tables 13 and 14.)

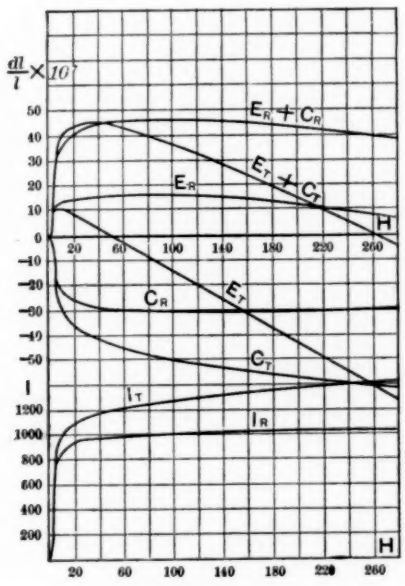

Plate 8.
Soft Iron: Tension 752.
(See Tables 15 and 16.)

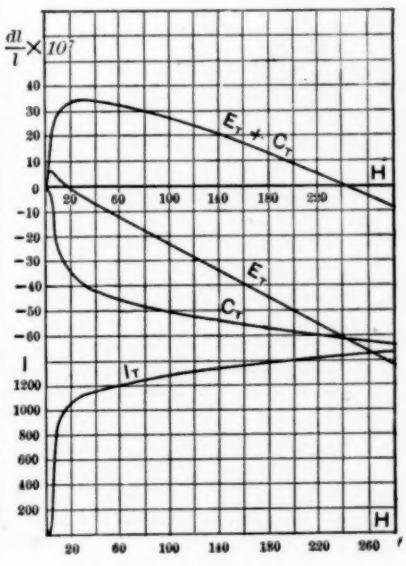

Plate 9.
Soft Iron: Tension 1720.

TABLE XIII.

Elongation data for soft, annealed iron wire under tension of 430 *kg. per square cm.*

$\frac{dl}{l} \times 10^7$ due to this tension = 1988.

H	E_T	E_R	H	E_T	E_R
.457	0.29	0.28	42.99	20.92	25.92
.915	0.853	0.68	46.48	20.38	26.11
1.51	1.71	1.42	50.68	19.62	26.19
1.82	1.99	1.82	54.82	18.76	26.19
2.42	3.41	2.84	60.08	17.61	26.19
2.88	4.55	4.09	67.00	15.92	26.22
3.43	6.27	5.69	76.50	13.41	26.40
3.93	7.97	7.40	91.95	9.96	26.72
4.62	10.52	9.68	101.4	6.95	26.72
5.26	12.39	11.37	113.2	3.99	26.72
6.12	14.51	13.64	129.5	− 0.57	26.60
7.31	17.06	15.91	139.8	− 3.13	26.60
9.14	19.21	18.18	152.4	− 6.27	26.40
10.40	20.19	19.33	163.7	− 9.68	25.60
12.21	21.32	20.44	178.9	−13.93	25.01
14.41	21.71	21.18	198.6	−19.21	24.42
17.54	22.78	22.77	228.5	−26.16	24.15
22.18	23.20	23.78	246.6	−31.00	23.02
25.32	23.20	24.20	262.5	−36.12	21.00
30.18	22.78	24.78	283.2	−42.99	18.77
33.48	22.20	25.01	308.3	−54.50	15.32
37.39	21.61	25.60			

CONCLUSIONS.

The investigation seems to have established the following laws for magnetization under conditions like those used in my work:

1. Any increase in the magnetic induction tends to lengthen the iron wire. As shown by the curves of elongation plotted to I, the relation between I and elongation is nearly the inverse of the relation between H and I shown in the ordinary induction curve, until well beyond the maximum value of μ, when it curves in an opposite direction on account of the field effect.

2. The magnetizing field tends to shorten the wire and the shortening due to this cause apparently has no limit. Up to a field of two or three hundred the shortening seems to be directly proportional to the field strength and then it seems to begin to approach asymptotically some limiting value.

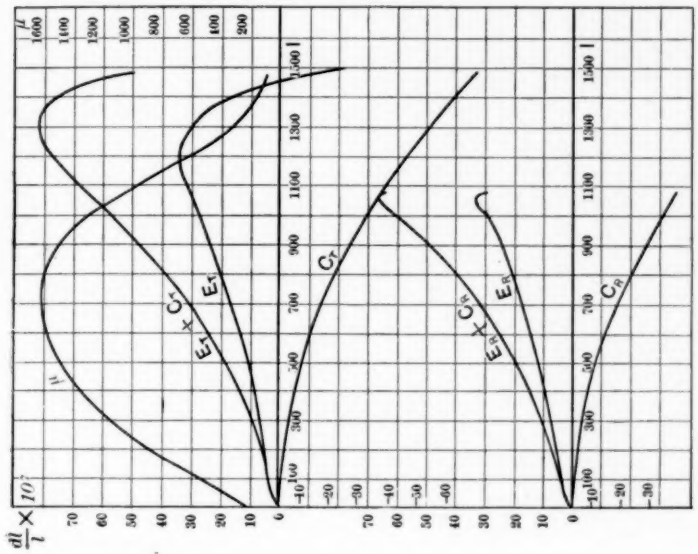

Plate 10.

Soft Iron: Tension 48.3. (See Tables 11 and 12.)

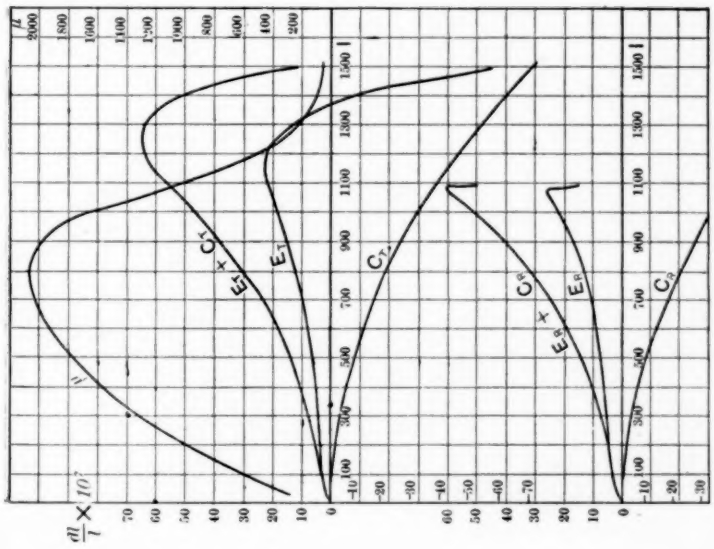

Plate 11.

Soft Iron: Tension 430. (See Tables 13 and 14.)

Table XIV.

Magnetization data for wire in Table XIII.

H	B	μ	I_T	I_R	C_T	C_R
1.46	464	318	36.8	8.09	.04	.0
1.92	752	392	59.7	23.5	.10	.02
2.29	1496	654	119.0	65.3	.42	.13
2.75	3235	1178	255.2	182.0	1.97	.98
3.39	5658	1672	442.5	347.5	6.00	3.57
4.58	9405	2060	748.5	634.5	16.6	11.9
5.22	10695	2060	852.5	733.8	21.4	15.9
6.08	11616	1910	924.2	820.0	25.3	19.9
7.32	12487	1710	994	880.0	29.3	22.9
9.04	12909	1430	1025	915.0	31.3	24.8
10.29	13270	1290	1055	937.5	33.1	26.0
12.09	13572	1120	1079	976	34.6	28.2
14.24	13864	973	1102	987.5	36.1	28.9
17.20	14197	826	1128	1008	37.8	30.1
21.60	14532	673	1154	1026	39.7	31.1
24.63	14775	600	1173	1042	41.0	32.1
29.28	14959	511	1187	1053	42.0	32.8
32.28	15082	468	1198	1053	42.7	32.8
35.81	15296	427	1213	1053	43.9	32.8
38.60	15399	399	1221	1053	44.5	32.8
41.00	15441	377	1225	1055	44.8	32.9
45.00	15605	347	1237	1056	45.8	33.0
52.63	15793	300	1252	1058	46.9	33 1
58.12	15938	274	1263	1058	47.7	33.1
68.60	16229	236	1285	1058	49.5	33.1
78.25	16408	210	1298	1059	50.6	33.1
93.15	16733	179	1324	1070	52.6	33.9
102.9	16903	165	1336	1070	53.6	33.9
114.3	17064	149	1348	1070	54.6	33.9
130.6	17411	133	1374	1074	56.8	34.2
140.0	17462	123	1380	1076	57.3	34.3
154.3	17674	115	1393	1076	58.5	34.3
166.5	17907	107.5	1411	1076	60.3	34.3
183.1	17983	98.0	1416	1078	60.7	34.4
203.8	18244	89.0	1436	1078	62.5	34.4
226.3	18366	81.3	1443	1078	63.3	34.4
249.2	18429	73.9	1446	1080	63.7	34.5
270.0	18790	69.5	1473	1080	66.3	34.5
297.5	18947	63.6	1483	1081	67.4	34.5
341.6	19422	56.8	1516	1092	70.8	35.3

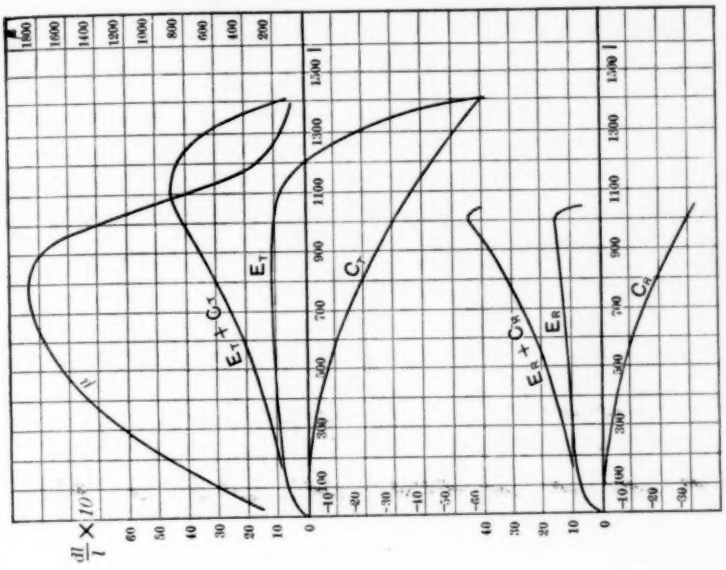

Plate 12.
Soft Iron : Tension 752.

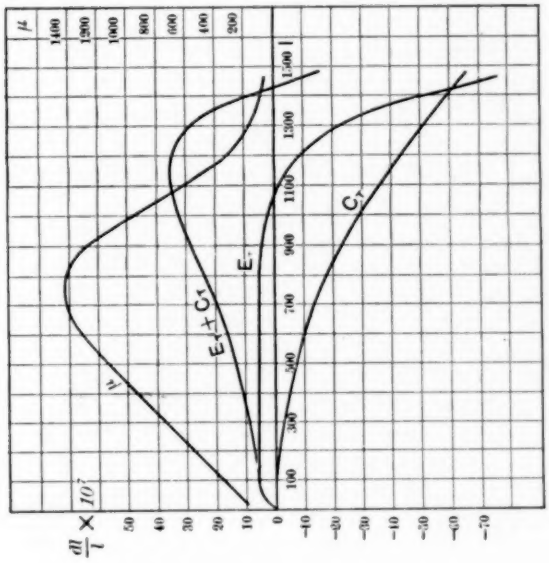

Plate 13.
Soft Iron : Tension 1720. (See Tables 17 and 18.)

3. The elasticity changes with the induction, the modulus being in some cases one-half per cent. greater at the highest magnetization than when the magnetization began. But the law of the change is unknown further than that the elasticity changes only as the induction changes.

In closing, I desire to acknowledge my indebtedness to Professor Rowland, not only for suggesting this investigation to me, but also for help and kind consideration at all stages of the work.

I am also greatly indebted to Dr. Ames, to Dr. Duncan and to Mr. H. S. Hering for valuable suggestions on many portions of the experimental work and for a very considerate interest in the whole investigation.

Explanation of the Tables and the Notation used in the Tables and Plates.

$H \equiv$ the magnetizing field.

$E_T \equiv \dfrac{\text{change of length}}{\text{length}} \times 10^7$ while H is on.

$E_R \equiv \dfrac{\text{change of length}}{\text{length}} \times 10^7$ after H is removed.

$B \equiv$ total induction or $4\pi I + H$.

$I_T \equiv$ magnetization of specimen with H on.

$I_R \equiv$ magnetization of specimen with H off.

$\mu \equiv$ magnetic permeability or $\dfrac{B}{H}$.

$C_T \equiv \dfrac{\text{change of length}}{\text{length}} \times 10^7$ that would be caused by a mechanical pressure $= \dfrac{B^2}{8\pi}$.

$C \equiv \dfrac{\text{change of length}}{\text{length}} \times 10^7$ that would result from a mechanical pressure $\dfrac{(4\pi I_R)^2}{8\pi}$.

If these compressive forces actually exist then the values of E_T and E_R computed from the observed changes are too small by C_T and C_R respectively and:

$E_T + C_T \equiv$ the real $\dfrac{\text{change of length}}{\text{length}} \times 10^7$ with H on.

$E_R + C_R \equiv$ the real $\dfrac{\text{change of length}}{\text{length}} \times 10^7$ with H off.

JOHNS HOPKINS UNIVERSITY, June 30, 1897.

THE DISCHARGE OF ELECTRIFIED BODIES BY THE X-RAYS.[1] II.

By Clement D. Child.

Theory.

THE results thus far given can be explained on the hypothesis given by Thomson and Rutherford.[2] It has been shown that the condition of the gas which is produced by the passage of X-rays through it, after lasting for an appreciable length of time, disappears. This condition is also rapidly destroyed by the passage of a current. It was, therefore, assumed that the gas through which the X-rays pass becomes dissociated, that the ions normally recombine after a short period, and that they recombine much more rapidly when a current passes through the gas. Since I shall have occasion to consider the theory somewhat in detail, the following sentences are quoted from Thomson and Rutherford:

"If we regard the gas as an electrolyte, then the passage of a quantity of electricity will destroy e/ε of the conducting particles, where e is the amount of electricity carried, and ε is the amount carried by one particle.

"Let n be the number of conducting particles in a unit volume of the gas, q the rate at which they are produced by the rays, an^2 the rate at which they disappear independently of the passage of the current, i the current through unit area of the gas, l the distance between the electrodes. Then we have

$$\frac{dn}{dt} = q - an^2 - \frac{i}{l\varepsilon}$$

so that when the state of the gas is steady

$$0 = q - an^2 - \frac{i}{l\varepsilon}.$$

[1] Concluded from the Physical Review, Vol. V., No. 4.

[2] Phil. Mag., 42, 392.

"If E is the difference of potential between the plates, U the sum of the velocities of the positively and negatively electrified particles when the potential gradient is unity, we have

$$i=\frac{n\varepsilon UE}{l} \text{ or } n=\frac{li}{\varepsilon UE}."$$

Substituting this for n in the above equation and solving for i we find

$$i=\frac{-\frac{1}{l\varepsilon}+\sqrt{\frac{4\alpha q l^2}{\varepsilon^2U^2E^2}+\frac{1}{l^2\varepsilon^2}}}{\frac{\alpha l^2}{2\varepsilon^2U^2E^2}}.$$

High Potential.—Now if E is very large $i=q\varepsilon l$ and the current is not determined by the potential nor by U, but is proportional to the rate at which conducting particles are formed. If, therefore, q is proportional to the square root of the density of the surrounding gas, we should have the current in the case of a large potential proportional to the square root of the density, and we should also have the limiting currents proportional to the same thing. This is approximately what was found to be the case.

Low Potential.—If, however, E remains very small while q becomes large, $i=\frac{\varepsilon UE}{l}\sqrt{\frac{q}{\alpha}}$. It is evident that both U, the velocity of the ions for unit potential gradient, and α, the rate at which the ions recombine when there is one per unit volume, may be functions of the density of the gas. As the density is increased we should expect U to decrease and α to increase. Such a change in either would account for the fact that with small potentials i becomes smaller as the density of the gas is increased.

Density for Maximum Discharge.—If now we make some assumption regarding the dependence of U, α, and q upon the density of the gas, it will be possible to find a value of d which will cause i to have a maximum value. It is apparent from the experimental work that q, the rate at which ions are formed, is roughly proportional to the square root of the density of the gas. Let us, therefore, say that $q=k\sqrt{d}$ where d is the density of the gas, and k is a quantity depending upon the intensity of the X-rays. If we assume that

$\frac{a}{U^2}=k'd^{\frac{3}{2}}$ where k' is a constant, it can be shown that $d_m=\frac{E}{2l}\sqrt{\frac{3}{kk'}}$ where d_m is the density that will cause i to be a maximum. I have no reason *a priori* for supposing that $\frac{a}{U^2}$ varies as the 3/2 power of d rather than according to some other power. This is assumed simply because it gives a value for d_m more consistent with the experimental results than any other supposition. It has already been shown experimentally that d_m varies approximately directly as E and inversely as the square root of k. I also plotted curves between i and d assuming that $\frac{a}{U_2}=k'd^{\frac{3}{2}}$ and $q=kd^{\frac{1}{2}}$. These curves agreed fairly well with those found experimentally, the dotted curves in Fig. 6 being the theoretical ones, and the smooth, the experimental.

If the assumption is made that $\frac{a}{U^2}=k'd^2$, then $d_m=C\frac{E^{\frac{2}{3}}}{k^{\frac{2}{3}}}$, where C is a function of l and k. This agrees nearly, but not fully, as well with the experimental results as the other value for d_m. The curves

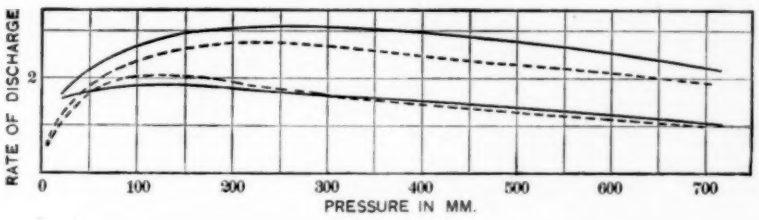

FIG. 6.

between i and d which we should then have also agree fairly well with the curves found. No other relation between d and $\frac{a}{U^2}$ explains the experimental results so well.

Rate of Recombining.—Thomson and Rutherford state that the rate at which the dissociated ions disappear independently of the passage of the current is proportional to the square of the number dissociated. That this is true rather than that they disappear at a rate proportional to the first power, appears from the experimental

results here given. If the later assumption be made, it can be shown that d_m will not depend at all on k, that is on the intensity of the rays. But experiment shows very clearly that d_m does depend on the intensity of the rays.

This same assumption can also be tested in another way. It has already been shown that if E is large, the current is proportional to q, the number of ions dissociated, but if E is small the current is proportional to the $\sqrt{q}$. It is most natural to suppose that q is proportional to the intensity of the rays, and thus it follows that a change in the intensity of the rays produces a greater effect on the rate of discharge when E is large than when E is small. This was tested and it was found that such was actually the case. With a difference of potential of 60 volts the fall in potential was 4 volts when the intensity was strong, and 2.1 when it was weak. With a difference of potential of 5 volts the fall was 1 volt when the intensity was strong, and .7 when it was weak. The ratio in the former case was 1.9 and in the latter 1.43. The difference in the two cases corresponds very closely with the difference which the theory calls for. This fact is an important one to bear in mind when one is measuring the intensity of X-rays by the rate of discharge which they produce.

Variation in Number of Discharges.—It is very evident that some more direct way of determining the relation between $\frac{a}{U^2}$ and d is desirable. It seemed possible that by varying the number of discharges per second through the tube, some idea might be obtained concerning this relationship. For this purpose a mercury interrupter, having a device for regulating the number of interruptions per second, was substituted for the magnetic interrupter.

It was first found that when the potential of the charged body was large, the rate of discharge was directly proportional to the number of interruptions up to 12 per second, but there was not this relationship when the potential was small. Curves are plotted in Fig. 7, showing the relation between the interruptions per second and the rate at which the electrified body is discharged in gas at different densities. These were taken with an initial potential of 5 volts.

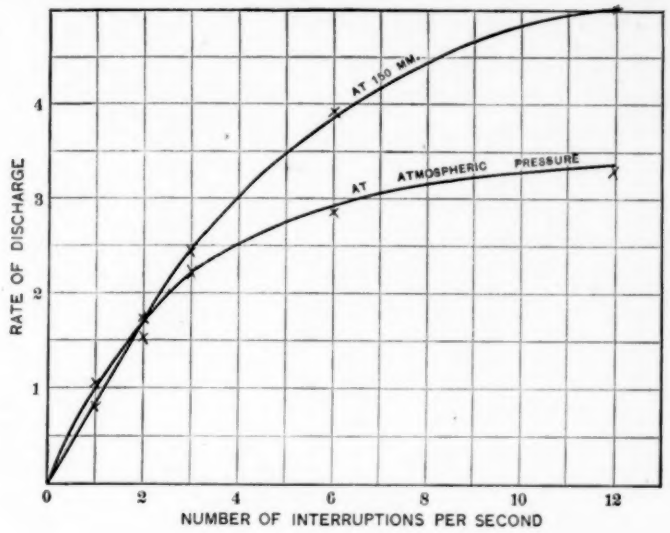

Fig. 7.

It will be noticed that, according to these curves, when a large number of interruptions in the primary circuit takes place, the discharge is more rapid for small densities of the gas than for larger. This is not the case when there are but two or three interruptions per second. It would appear, therefore, that if we should vary the density of the gas, using but few interruptions per second, there would be no maximum current, even though the potential were small. This was found experimentally to be the case.

It is evident that these results are of considerable importance, if a correct explanation for them can be found. In the first place, it is necessary to explain the fact that the rate of discharge is proportional to the number of interruptions per second when the potential is large, and is not proportional when it is small. It has been estimated that the ionized condition of the gas continues for about $\frac{1}{10}$ of a second. It is evident, moreover, that the time will be shorter if a large discharge takes place, and the normal rate of reassociation is increased by the passage of the current through the gas.

If, then, the discharge due to one flash of the rays from the tube is finished before another one occurs, the total discharge will be directly proportional to the number of flashes which are sent out. If,

however, one such discharge is not completed before another occurs, such a direct proportion does not hold. For the rate at which the ions normally recombine is assumed to be proportional to the square of the total number present. When few ions are left from a given flash of the rays, their rate of recombining is small, as compared with the number still present, and the current which they carry before they completely recombine will be appreciable.

If, however, a new supply of ions is suddenly added, the rate of recombining is much larger, the ions left over from the first supply are quickly recombined and the amount of electricity carried by them is inappreciable. In this case the total amount of electricity carried by any one supply of ions is less than it would have been if each supply had acted independently. This would explain the fact that the rate of discharge is no longer proportional to the number of interruptions in the coil, when this number is large.

If this be true, then it appears from the curves in Fig. 6 that at atmospheric pressure the discharges begin to interfere with each other when there are 2 per sec., but at a pressure of 150 mm. they do not interfere with each other until there are about 6 per sec. In other words, they more quickly recombine at lower pressures.

This may be caused either by the rate at which they normally recombine being greater at the lower pressure, or by an increase in the rate at which they recombine due to the current. However, if it were due to the fact that they normally recombine more rapidly, the ions would not have as great a chance to carry electricity, and the amount of electricity discharged would therefore be less at the lower pressure. This is not the case.

We are, therefore, driven to the other explanation, namely, that the current causes the ions to recombine more rapidly when the pressure is low. If the velocity of the ions were greater at the lower pressure, then the ions would carry more current and would also be recombined more rapidly. This explanation would, therefore, account for both of the observed facts.

Modification of Theory—Action Discontinuous.—Let us, therefore, return to the mathematical part of the theory. Thomson and Rutherford assumed that the number of dissociated ions in the gas remained constant, being replenished by the action of the X-rays at a

uniform rate equal to the rate at which they recombined. It would appear from my last set of curves that when the potential is large, the action of the ions is not continuous, but that the gas resumes its normal condition after each discharge of the tube. The assumption in this case is not only not in accordance with the facts, it is not even an approximation. It will, therefore, be of interest to work the theory through on a more correct assumption in this particular.

A graphical representation of what takes place can be given by curves similar to those in Fig. 8. The ordinates correspond to the number of ions present, the abscissæ to the time. Since the rate of discharge is proportional to the number of ions present, this rate at any instant is also proportional to the corresponding ordinate. The total discharge is proportional to the area of all the curves.

FIG. 8.

First let us find the quantity of electricity discharged by one supply of dissociated ions. Let b equal the rate at which the ions recombine due to the action of the current. Then $\frac{dn}{dt}=-an^2-bn$. Solving this differential equation we find that $n=\frac{n_0 b\varepsilon^{-bt}}{an_0(1-\varepsilon^{-bt})+b}$ where n_0 is the number of the ions when $t=0$. This would be the number due to one flash of the X-rays. Now $i=\frac{\varepsilon UE}{l}n$ and if Q represents the total quantity of the electricity discharged by this one supply of ions,

$$Q=\frac{\varepsilon UE}{l}\int_0^\infty ndt=\frac{\varepsilon UEn_0 b}{l}\int_0^\infty \frac{\varepsilon^{-bt}}{an_0(1-\varepsilon^{-bt})+b}\,dt=\frac{\varepsilon UE}{la}\log\frac{an_0+b}{b}$$

$$\text{But } bn=\frac{i}{l\varepsilon} \text{ and } i=\frac{\varepsilon UE}{l}n \therefore b=\frac{UE}{l^2} \text{ and } Q=\frac{\varepsilon UE}{la}\log\frac{an_0+\frac{UE}{l^2}}{\frac{UE}{l^2}}$$

If this be evaluated for a large value of E, we find that $Q=n_0\varepsilon l$. If there are m discharges per sec. and each one finishes its action be-

fore another one begins, the total discharge per sec. is $mQ=mn_0\varepsilon l$. This agrees identically with the result given by Thomson and Rutherford when we remember that q must be considered equal to mn_0. This also agrees with the fact that in this case the rate of discharge is directly proportional to the number of interruptions per second.

If now we go back to the expression for Q and evaluate it for a small value of E, we find that

$$Q=\frac{\varepsilon l n_0 \frac{UE}{l^2}}{an_0+\frac{UE}{l^2}}$$

and if $\frac{UE}{l^2}$ is very small as compared with an_0

$$Q=\frac{\varepsilon UE}{al} \text{ and } Qm=\frac{m\varepsilon UE}{al}.$$

Thomson and Rutherford found $i=\frac{\varepsilon UE}{l}\sqrt{\frac{q}{a}}$ when E was small.

The value for Qm would seem to be incorrect in that it is independent of n_0. It must be remembered, however, that this does not hold except when n is very large and E very small, and the action is entirely discontinuous. There would be the greatest difficulty in detecting the rate of discharge under these circumstances. It can not be claimed that any experimental data found by myself were taken under circumstances which would satisfy these conditions.

Action Nearly Discontinuous.—Let us next consider the case when the action is nearly but not quite discontinuous. In this case the ions would be nearly recombined before a second discharge occurred. The few remaining uncombined when the second group of ions was supplied would recombine so quickly, due the presence of additional ions, that their presence could be neglected. In that case

$$Q=\frac{\varepsilon UE}{l}\int_0^{t_1} ndt$$

where t_1 is of such a value that ε^{-bt_1} is nearly equal to zero. If this be integrated and evaluated when E is small, we find

$$mQ=m\frac{\varepsilon ln_0\frac{UE}{l^2}}{an_0+\frac{UE}{l^2}}\left(1-\varepsilon^{-\frac{UE}{l^2m}}\right), \text{ since } t_1=\frac{1}{m}.$$

It is apparent that this quantity is not proportional to m because of the term $\varepsilon^{-\frac{UE}{l^2m}}$. This term will become larger as U becomes smaller, and since there is greater deviation from the law of strict proportionality in the case of discharge in a gas of greater density, one is led to the conclusion that it becomes smaller as the density of the gas becomes greater.

In the case that $\left(1-\varepsilon^{-\frac{UE}{l^2m}}\right)$ differs appreciably from 1, it is evident that this factor will cause Q to increase as U increases. These two conclusions are the same that we have derived by more general consideration of the curves in Fig. 7.

Action Nearly Continuous.—It would be desirable to consider the case when the action of the ions is far from being discontinuous and is not completely continuous. This can be done by making one or two assumptions. The value found for i in this case when E is small is practically the same as that found by Thomson and Rutherford. But it is doubtful whether the theoretical work is at present of any value. It is possible to have a theory with more assumptions than facts, and in this case it certainly would be well to test those assumptions made concerning the rate of normal reassociation and the rate due to the action of the current, by some more direct means than has yet been employed, before a more elaborate theory is constructed.

It is thus evident that I am leaving this subject in a very incomplete condition. Both the fundamental assumptions might be tested by more direct methods and the consequences could be worked out more in detail. However, circumstances necessitate a postponement of further work by myself on this subject for the present. It, therefore, seems better to publish what has already been done and trust that the work may eventually be more fully completed.

PHYSICAL LABORATORY OF CORNELL UNIVERSITY.

MINOR CONTRIBUTIONS.

AN EXPERIMENTAL RESEARCH ON GRAVITATIONAL PERMEABILITY.

BY LOUIS W. AUSTIN AND CHARLES B. THWING.

THERE are certain suppositions ordinarily accepted as to the physical properties of gravitation. In general, it is supposed that its intensity depends only upon the amount of matter in, and the geometrical configuration of the attracting masses and that it is independent

I. Of the physical conditions, *i. e.*, solid, liquid, gaseous, amorphous or crystalline.

II. Of the chemical combinations in which the masses happen to exist.

III. Of the temperature of the masses or the medium between them.

IV. It is also supposed that there is no directive character to the field, and

V. That the attraction is quantitatively independent of the intervening medium.

That these suppositions are at least approximately true is very probable, but at the same time the degrees of accuracy of these approximations seem to offer suitable problems for experimental research.

There has been extremely little work done on the question of the variability of gravitation compared with that expended on the determination of the gravitation constant and its attendant problem of the mean density of the earth. Of the above supposition, II.[1] and IV.[2] are, we believe, the only ones which have been investigated directly. The last supposition, No. V., is the subject of investigation in the present paper, which is to be considered only as a preliminary notice of the work, which is still in progress.

That there is nothing comparable to specific inductive capacity or magnetic permeability in gravitation can not be proved by astronomical observations, as here there is practically but one medium involved, the ether. Neither does the fact that no screen appears to affect the attraction of the earth for terrestrial objects prove the supposition. For, since the mass of the earth may be considered as acting as though concentrated at its center of figure, the thickness of any screen which might be introduced between the earth and the attracted body would be in-

[1] Landolt Zeitschrift f. Phys. Chem., Vol. XII., 1894, p. 1.

[2] A. S. Mackenzie, PHYSICAL REVIEW, Vol. II., March, 1895, p. 321.

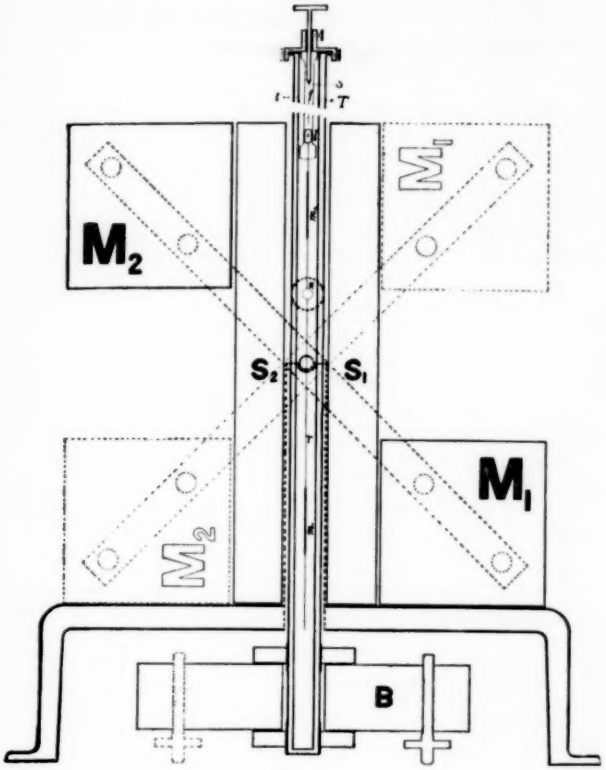

Fig. 1.

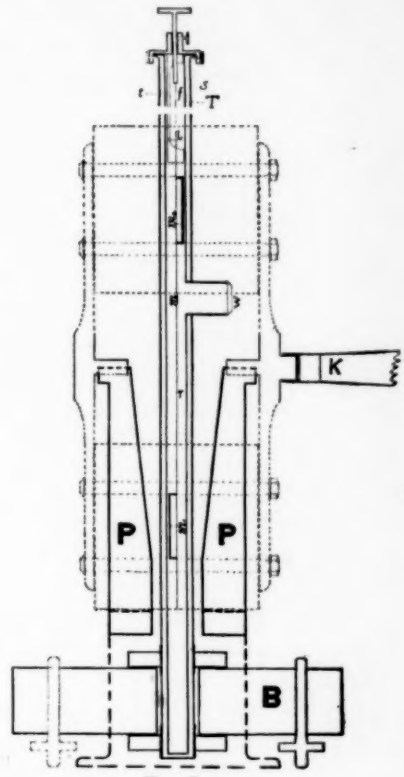

Fig. 2.

finitesimal compared with the total distance between the two masses, and any disturbance in the earth's attraction would be too slight to be observed. Then, too, as has been noted by Professor Poynting,[1] the earth's spherical form might render observations inconclusive, for, just as a series of concentric electrified dielectric shells would have the same external field, whatever their dielectric constant, so the earth might have the same external field whatever the influence that its material might have on the transmission of the gravitational force. The fact, however, that nothing corresponding to the tangential law of refraction of electrical lines of force has been observed in gravitation shows that the supposition is at least approximately true. Again, the fact that the constant of gravitation as derived from cylinders in the work of Wilsing,[2] and,

[1] J. H. Poynting, The Mean Density of the Earth, p. 7; Charles Griffin & Co., London, 1894.

[2] J. Wilsing, Publicationen des Astro.-Phys. Observ. zu Potsdam, No. 22, VI. Band, II. Stück, 1885, p. 35; also No. 23, VI., III. Stuck, 1889, p. 133.

later of Boys,[1] is nearly the same as that derived from spheres, shows that the variation of gravitational permeability is not large. Small as the influence of the medium must be, it seems, nevertheless, worth while to look for it.

The Apparatus.

The work is being carried on in a basement room in the Physical Laboratory of the University of Wisconsin, a room especially well fitted for this purpose, as its temperature is remarkably constant, seldom varying more than 0.5° C. in four hours and often remaining constant within 1° or 2° for weeks at a time. In this room a pier of the most substantial kind was built solid of brick, 2 m. square, on a heavy foundation of stone laid in sand 1 m. below the floor of the room and having on top a stone slab 2 m. square and 20 cm. thick.

The gravitation instrument is of the general form devised by Professor Boys,[2] and the main features are shown in Figs. 1 and 2, which are vertical cross sections through the center of the instrument. Two masses of gold wire (m_1 m_2) each 4 cm. in length, 0.08 cm. in diameter and weighing 0.4 g. are attached to opposite sides of a fine tube of glass (r) at a vertical distance apart of 19 cm. and 0.4 cm. from the axis of rotation. These serve as the attracted weights. To the upper end of the glass tube a loop of fine wire (l) is attached, to the top of which the quartz fiber (f) is fastened. The suspended system weighs 0.98 g. The fiber hangs from a rod which passes through a removable cap, as is usual in galvanometer suspensions. To the under side of this cap is soldered a thin flat supporting rod (s), from the bottom of which a pin projects through the loop (l). This pin and loop make a convenient support for the needle while the fiber is being attached, prevent the fiber from being broken when the system is being lowered into the tube of the instrument, prevent undue rotation when the system is first set free, and save it from damage by falling in case the fiber breaks. Readings are taken from a mirror (m) on a millimeter scale 4.5 meters distant.

To guard against magnetic disturbances only "non magnetic," that is weakly magnetic, material was used in the instrument. Electrostatic disturbances are prevented by the metal case, which is connected to earth. The chief disturbances in any very sensitive instrument of this type are due to tremors and to air currents set up in the tube by temperature changes in the room. To obviate the latter difficulty as far as possible, the tube (t), which had a diameter of 1.4 cm., is enclosed in a second tube (T) having a diameter of 2 cm. and the space between filled with a poor conductor of heat, paraffine. This arrangement of outer and inner conduct-

[1] C. V. Boys, Phil. Trans., 186 A., 1895, p. 1.
[2] C. V. Boys, Nature, Vol. XLI., Dec. 19, 1889, p. 155.

ing tubes with a middle non-conducting layer secures good uniformity of temperature in the interior. This is essential since non-uniformity ordinarily produces the vexatious air currents which form the greatest obstacle to accurate measurements in instruments of very delicate suspended systems. The freedom from these disturbances is such, that an open window with a cold winter wind blowing in within two meters of the instrument hardly disturbed it at all, provided the top was made tight with a rubber washer. The double tube just described is mounted upon a marble base provided with leveling screws.

The deflecting masses ($M_1 M_2$) consist of two decimeter cubes of lead fastened together by two bars of brass, from which project trunnions which rest in bearings at the top of the two upright posts ($P_1 P_2$). These form part of a heavy frame of cast brass entirely separate from the instrument itself. By means of a wooden rod having a key (k) at its end the weights can be turned instantly from the position shown by the solid lines to that shown by the dotted lines, without in the least jarring the needle. The weights can also be left horizontal or removed entirely when it is desired to do so. For the purpose of testing the screening or intensifying effect, if such there be, of intervening bodies a space of 3.5 cm. was left on each side between the lead cubes (M) and the tube. In these spaces are inserted screens of the substance to be investigated. When liquids are used they are contained in rectangular copper cans. These screens ($S_1 S_2$) are 3 cm. thick, 10 cm. wide and 29 cm. high. Since they are symmetrically placed in respect to the needle, their combined effect in producing a deflection of the latter is practically zero.

Method of Observation.

On account of the enormous damping, due to the narrowness of the tube in which the needle hangs, it is found impossible to determine the point of rest by the methods of swings. It is therefore necessary to wait after each reversal of the lead weights until the needle has come to rest (about ten minutes), when readings are begun and continued every half minute for five minutes. The work has been done almost entirely at night in order to gain greater freedom from vibration, which has been by far our worst source of disturbance. Under favorable circumstances, however, and especially late at night the steadiness of the needle is remarkable, being quite as great as that of any very sensitive galvanometer. It has often happened that during the whole ten readings which make up a set the greatest variation from the mean has not exceeded 0.1 mm. on the scale.

We found that there were slow and in general steady changes in sensitiveness for many weeks and in some cases months after the needle was put in place. This was most probably due to changes in the needle itself

caused by the glass slowly relieving itself from the strain under which it was almost unavoidably put when the gold weights were cemented in place. We have also recently noticed a change in the sensitiveness with the temperature, the sensitiveness being increased when the temperature rose. This also we believe to be due to a bending of the needle caused by the unequal expansion of the glass and gold. To remedy this in future work platinum weights will be used in place of the gold, unless platinum shall prove too magnetic for the purpose. We have endeavored thus far to make the instrument as free as possible from magnetic disturbance, not even using iron tools in making the needle, but all our efforts have only succeeded in producing a needle which, with our present fiber will not be deflected more than 10 cm. on the scale when a strong bar magnet is brought close to the tube of the instrument.

During the course of our work we have used a number of quartz fibers of different degrees of torsional rigidity. One used in the early part of the work gave a deflection of 107 cm. on a scale 4.5 m. distant, with a time of swing of between six and seven minutes, the exact time of swing being extremely difficult to determine on account of the great damping already mentioned, which brought the needle practically to rest after the first complete swing. Most of the observations thus far have been taken with two fibers, which gave deflections of about 26.5 cm. and 36 cm. respectively with times of swing of about 170 sec. and 190 sec. The first of these fibers, strange to say, broke during the night when not in use and when bearing much less than the weight for which it had been tested before being put in place.

There have been several changes in our methods of work since the earlier observations. At first, with the intention of avoiding the frequent disturbance of the instrument by the shifting of the screens, observations were taken for a week without the screens, then for a week with the screens, then for a week without the screens and so on. But, as it was soon found that the effect of changing the screens seemed to be entirely over in twenty-four hours and as the slow change in sensitiveness seemed to be a more probable source of error, in the later work the screens have been shifted every day or every second day.

There are three properties of matter to which we are looking as likely to be connected with an effect on gravitational permeability. First, density on account of its close connection with gravitation itself. Second, specific inductive capacity on account of the great modifications of the ether which must accompany its variations. Third, for the same reason, magnetic permeability. In the first class lead, mercury and zinc have been examined as screens. In the second, water on account of its high specific inductive capacity was extensively experimented upon. Alcohol and glycerine were also examined. In the third class no observations of any

value have been made on account of the magnetic qualities of the needle, although iron is perhaps the one substance that we are most anxious to examine.

In the following table is given one of the series of observations with and without the water screens.

TABLE I.

WATER.

	Without Screens.	With Screens.	Temp.
1		35.97	24.0
2	36.01		24.1
3		36.07	24.5
4	36.00		24.6
5		36.05	24.5
6		36.00	24.5
7	35.98		24.2
8	36.11		25.0
9		36.11	24.8
Av.	36.025	36.040	

The difference, 0.015 cm is less than 0.05% of the total deflection. The table also shows the admirable constancy of the temperature in the observing room.

Below are given the three sets of half minute observations from which deflection 9 was calculated:

	Pos. I.	Pos. II.	Pos. I.		Deflection.
	55.79	91.84	55.65		
	.82	.82	.62		
	.82	.82	.62		
	.80	.82	.62	55.79	
	.75	.80	.62		91.81...36.11
	.73	.77	.61	55.72	
	.77	.78	.64		
	.78	.80	.61		
	.75	.79	.63		
	.78	.80	.60		
Av.	55.79	91.81	55.62		

Several other series nearly or quite as satisfactory as this given have been taken on water as well as on the other substances before mentioned, though in many of them the slow changes in sensitiveness are more apparent. The results given serve to show how well the instrument behaves under favorable circumstances. No attempt has been made to apply the method of probable error to the determination of the

accuracy of the observations, as disturbances in general seem to tend to move the needle much more in the one direction than in the other, which fact removes the errors from the class which can be treated by that method.

Though the exact degree of accuracy of the observations cannot be ascertained, it can be stated conclusively that the error in our final determinations can, in the case of none of the substances experimented upon, with the exception of iron, much exceed 0.2%. We can thus sum up the results of our work thus far as follows: With screens of lead, zinc, mercury, water, alcohol or glycerine occupying one-third of the distance between the attracting masses and the needle, the change produced in the attractive force is certainly less than 0.2% of the total attraction. In the case of the iron screens no certain conclusions can be drawn, as the presence of the iron modified the magnetic field to such an extent that the sensitiveness of the somewhat magnetic needle was almost certainly changed. The change in the gravitational force, if it exists at all, is, we feel sure, less than 1% of the total force.

In the future work which is to be carried on, it is intended to make use of an instrument with a larger tube, so as to reduce the damping and allow the time of swing and relative sensitiveness to be determined with accuracy. With this it is hoped that the questions of the effect of temperature and of the iron screens can be attacked successfully. The work already done will also be repeated, with the hope of reducing the estimated error to less than 0.1%.

PHYSICAL LABORATORY OF THE UNIVERSITY OF WISCONSIN, June, 1897.

ON THE BEST RESISTANCE FOR A SENSITIVE GALVANOMETER.

BY FRANK A. LAWS.

IT is well known that in the practice of most methods of electrical testing, the precision attainable depends on the proper adjustment of the galvanometer resistance to the work in hand, and that in planning new work a solution for the best galvanometer resistance is always made.

It is our purpose to derive a solution under conditions other than those usually imposed.

To render the discussion more complete, a statement of the usual solution of the problem will be given. It is as follows: If a simple circuit, consisting of a battery, a resistance and a galvanometer, be arranged, the deflection of the instrument will be a maximum when the galvanometer resistance is equal to that of the remainder of the circuit. In obtaining this solution two assumptions are made:

First. That the coils have fixed dimensions.

Second. That the ratio of the diameter of the covered to that of the bare wire is the same for all sizes.

That this second assumption is incorrect is shown by the fact that the ratio for a No. 20 B. & S. guage double silk-covered wire of the American Electrical Works is 1.12, while for a No. 40 wire it is 2.77.

In the course of the discussion we shall use the following symbols:

G, the galvanometer constant, field at centre of coil per unit current.

I_G, galvanometer current.

D, deflection of instrument.

V, volume of the coil.

b, linear constant of the coil, $b = k(V)^{\frac{1}{3}}$.

R_G, the galvanometer resistance.

R, the resistance of the circuit external to the galvanometer.

C, the diameter of the covered wire.

B, the diameter of the bare wire.

$y = C/B$.

w, the resistance per unit volume of the wire.

n, the number of turns per unit area.

E, E.M.F. of battery.

k, constants.

We shall assume an indefinitely short needle, and for the present retain the first assumption.

The usual demonstration is based on the equation,

$$G = k\sqrt{R_G}.$$

This equation requires correction. It is easily shown that

$$G = 4\pi nb\,(\varphi \cdot s),$$

where $(\varphi \cdot s)$ is a function the shape of the coil. As the same bobbin is to be used in all cases, we have

$$G = k_1 n = \frac{k_1}{C^2} = k_2 \frac{\sqrt{w}}{y} = k_3 \sqrt{\frac{R_G}{y^2}}.$$

To apply this to the typical circuit we have

$$I_G = \frac{E}{R + R_G}, \quad D = k_4 I_G G = k_5 \frac{E\sqrt{\frac{R_G}{y^2}}}{R + R_G}.$$

Differentiating and solving for a maximum, we have

$$R_G = \frac{R}{\frac{2}{y^2 \frac{d\left(\frac{R_G}{y^2}\right)}{dR_G}} - 1}$$

If y be constant, this reduces to

$$R_G = R,$$

the relation usually given.

This general formula was first given by Professor Silas W. Holman, In order to employ it the properties of the wire must be found by winding experimental coils.

The assumption of fixed dimensions will now be discarded and attention given to the galvanometer constants of a family of similar instruments. For coils of rectangular cross-section we have, referring to the diagram:

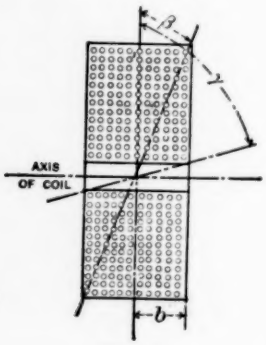

Fig. 1.

$$G = 4\pi nb \log_e \frac{\cot \frac{\beta}{2}}{\cot \frac{\gamma}{2}}.$$

For coils of this shape,

$$\varphi \cdot s = \log_e \frac{\cot \frac{\beta}{2}}{\cot \frac{\gamma}{2}}.$$

For instruments of the same family, β and γ constant, we have:

$$G = k_6 nb = k_6 \frac{b}{yyB^2}$$

$$b = k_7 \left(\frac{R_G}{w}\right)^{\frac{1}{6}},$$

$$y = \frac{k_9}{B^2 w^{\frac{1}{3}}},$$

$$\therefore G = k_{10}\left(\frac{w^{\frac{1}{6}}}{y}\right) R_G{}^{\frac{1}{6}}.$$

This shows that the sensitiveness of similar galvanometers wound to various resistances with the same size of wire, is proportional to the cube root of the galvanometer resistance. Also for a family of instruments of the same resistance the best size of wire will be that for which $\frac{w^{\frac{1}{6}}}{y}$ is a maximum. By winding experimental coils we obtain data for determining this function, a table of values of which is given below.

WIRE OF AMERICAN ELECTRICAL WORKS.

B. & S. Gauge No.	Single, Silk-covered. $\frac{w^{\frac{1}{2}}}{y}$	Double, Silk-covered. $\frac{w^{\frac{1}{2}}}{y}$	Ratio.
20	.94	.87	1.08
26	1.40	1.12	1.25
30	1.81	1.27	1.42
34	2.21	1.34	1.65
36	2.19	1.33	1.65
38	2.10	1.28	1.64
40	....	1.15	....

The results are plotted in Fig. 2.

The best size for both single and double-covered wire is seen to be No. 34. This result of course applies only to the particular make of wire examined.

We see that there may be two instruments of a family having the same resistance which will give equal deflections with the same current.

We are now in position to find the best galvanometer resistance. We have

$$D = k_{10}\left(\frac{w^{\frac{1}{2}}}{y}\right)\frac{R_G^{\frac{1}{2}}}{R + R_G}.$$

Considering for the moment that all the instruments are wound with the same size of wire, we have a maximum value of D when

$$R_G = R/2.$$

Therefore the best galvanometer of the family is one which is wound with a No. 34 wire to a resistance equal to one-half that of the remainder of the circuit.

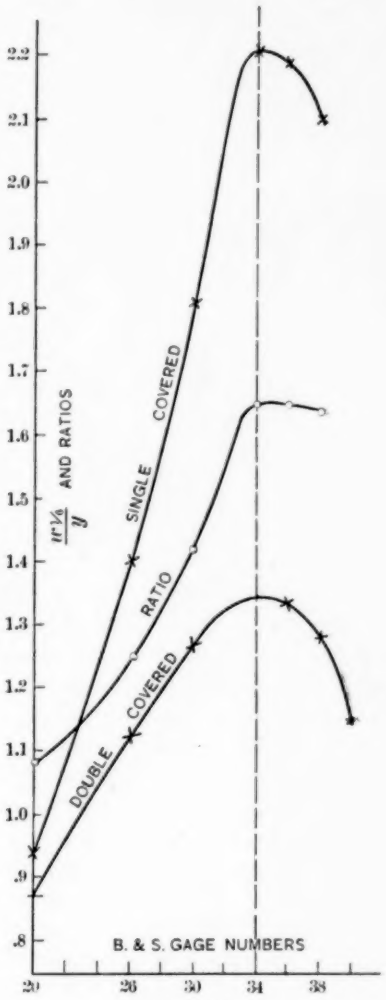

Fig. 2.

Relative galvanometer constants for similar coils wound to the same resistance with various sizes of silk-covered wire.

We will now consider two special examples:—

The coils of two-spool reflecting galvanometers very commonly have a volume of about eight cubic inches. Suppose the resistance external to the instrument to be 10ω and that double-silk-covered wire is to be used;

then by the ordinary rule the ideal galvanometer resistance is 10ω. In accordance with our first deduction No. 20 wire will give the best winding. R_G will then be 6ω;

$$D = k\, I_G\, G = k_1\, I_G\, nb;$$

$$D_{26} = k_1 \frac{E}{16.} 775.\ (8.)^{\frac{1}{2}}.$$

For the instrument wound with No. 34 wire we have $R_G = 5.\omega$, corresponding to a volume of coil of .026 cubic inches;

$$D_{34} = k_1 \frac{E}{15}.\ 7780.\ (.026)^{\frac{1}{2}};$$

$$\frac{D_{34}}{D_{26}} = 1.6 \text{ appox.}$$

The deflection of an instrument similar to the first but designed in accordance with our suggestion, will be 60% greater than that of the instrument wound in accordance with what is called the solution for the best galvanometer resistance.

The coils of eight cubic inches would have a diameter of about 2.6″ and an axial breadth of about 1.5″. The approximate dimensions of the second coil would be diameter .4″, breadth .25″. The weight of wire in the first case would be 30. oz., in the second, .032 oz.

The results we have just obtained are in emphasis of the plea of Professor Boys for small instruments. In this connection we may observe that if we have two similar galvanometers of equal resistance the first having a constant G_1, and wound with wire for which the ratio of the diameter covered to bare wire is y_1, while these quantities for the second instrument are G_2 and y_2, and if the linear dimensions of the first are N times those of the second, then,

$$\frac{G_2}{G_1} = \sqrt{N}\left(\frac{y_1}{y_2}\right)$$

The factor $\frac{y_1}{y_2}$ shows that there is a limit below which it is unprofitable to reduce the size of the instrument, this being the size for which a No. 34 wire is appropriate.

For galvanometers originally wound with wire finer than No. 34, the more sensitive instrument would be the larger.

We will now consider the case of a Wheatstone bridge where the resistances of the arms are small and nearly equal. Let them be of about one ohm each. Such an arrangement would be used in standardizing one-ohm coils or in bolometric work.

The ordinary solution gives one ohm as the best galvanometer resistance. If the coil has a volume of 8 cubic inches, the winding should be of No. 16 wire making $R_G = .88\omega$. We will suppose the E. M. F. of the battery to be 1 volt, its resistance 1 ohm, and that the bridge is out of balance by $\frac{1}{1000}$ ohn; then with due regard to carrying capacity we have

$$I_G = \tfrac{1}{15000}. \text{ approx.}$$
$$D_{16} = k_1\, 325.\ (8.)^{\frac{1}{3}}\, \tfrac{1}{15000}.$$

If wound as here indicated, we should have

$$R_G = .5\omega \quad I_G = \tfrac{1}{12000}.$$
$$D_{34} = k_1\, 7780.\ (.0026)^{\frac{1}{3}}\, \tfrac{1}{12000}.$$
$$\frac{D_{34}}{D_{16}} = 2.04$$

The deflection may be doubled by properly designing the galvanometer. This result is for double-covered wire. If single-covered be used, we should multiply by the ratio factor 1.65, giving

$$\frac{D_{34}}{D_{16}} = 3.36;$$

showing that by the use of a proper galvanometer wound with single-covered wire the sensitiveness of the bridge may be increased about 200 per cent.

The gain indicated in the examples is much greater with very low than with high resistance instruments, the former as usually wound departing more widely from the conditions here imposed.

Of course the numerical results indicate chiefly the order of magnitude of the gain for it would be impossible to reduce the size of the instruments as much as is here indicated. Also we have not considered the magnetic system, or the fact that the space at the centre of the coil must be large enough for free movement of the needle and that turns in very close proximity to a finite needle produce less than the effect indicated by the strength of field at the centre of the coil.

Our conclusions are, that if an existing arrangement of apparatus be taken, we can increase its sensitiveness by replacing the galvanometer by a similar instrument wound with wire of a particular size to a resistance equal to one-half of that of the remainder of the circuit. This best size of wire is that for which $\frac{w^{\frac{1}{6}}}{y}$ is a maximum.

For low resistance instruments the reduction in size renders magnetic shielding possible without undue clumsiness.

ROGERS LABORATORY OF PHYSICS, MASSACHUSETTS INSTITUTE OF TECHNOLOGY,
July, 1897.

A LECTURE EXPERIMENT TO SHOW THE INFLUENCE OF ULTRA-VIOLET LIGHT ON THE SPARK DISCHARGE.

BY ERNEST MERRITT.

THE discovery of the influence of the Röntgen rays and Becquerel rays in accelerating the discharge of electrified bodies naturally attracts attention to other phenomena of a similar character. Most prominent among these is the effect of ultra-violet light in discharging negatively charged metals. This effect being also an important one on its own account, I have tried during the past year to show the more important phenomena to one of my classes.

Thanks to the sensitive and reliable amalgam cells devised by Elster and Geitel,[1] it is an easy matter to show to an audience the more recent experiments bearing upon the phenomenon. The effect of ordinary sources of light, such as the gas flame, is readily shown by these cells, and no difficulty is experienced in determining the relative effect of different wave-lengths by moving the cell through the visible and ultra-violet spectrum formed by a prism.

In discussing the influence of ultra-violet light it is natural, however, to go back first of all to the original observations which led to the discovery of the phenomenon by Hertz. It is possible to show these experiments to three or four persons without any special precautions; but I experienced considerable difficulty in making them visible at a distance. This was particularly true in the case of the first experiments of Hertz, where it was found that the maximum sparking distance of a resonator or induction coil is increased by illuminating the terminals with ultra-violet light. The trouble is that the spark gap must be quite short in order to be sensitive, and this in itself makes it difficult to see the sparks at any great distance. The light used in illuminating the terminals is also troublesome, since it has a tendency to make the spark itself appear less bright by contrast. With the arc light, which forms by far the best and most convenient source for these experiments, I found it almost impossible to make the effect of illumination distinctly visible to more than fifteen or twenty at once. The following method of showing the fundamental phenomena has the advantage of enabling the experiments to be readily seen by a large audience.

To show the influence of the ultra-violet light in increasing the maximum sparking distance the arrangement of apparatus shown in Fig. 1 was used. In series with the spark gap, A, which is to be subjected to

[1] Wiedemann's Annalen, Vol. 42, p. 564. Such cells may be obtained in a variety of forms from L. Müller-Unkel in Braunschweig.

the influence of ultra-violet rays, is placed a Geissler tube G. In multiple with the spark gap and tube is placed a second spark gap, B, which is screened from the action of the rays. This second spark gap should be capable of rather delicate adjustment. By altering its length we may cause the spark to pass at B instead of at A, and yet have the difference in the resistance of the two paths so slight that when ultra-violet light is allowed to fall upon A sparks will immediately pass through that branch of the circuit rather than through B. The Geissler tube will then be illuminated each time a spark passes at A. The brilliancy of the Geissler tube discharge is so much greater than that of the small spark that it can be seen at a very considerable distance. A comparatively simple arrangement of screens is sufficient to protect the eye of the observer from the arc light used in illuminating the spark gap.

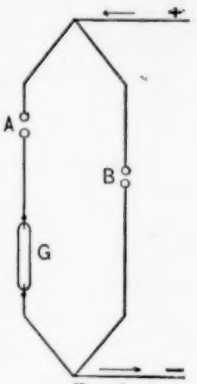

Fig. 1.

The arrangement of apparatus above described is especially convenient in showing the opacity or transparency of various substances for the active rays. Paper, mica and glass—in fact, almost all substances—screen off the effect of the arc completely, while quartz and gypsum are fairly transparent. If a spectrum is formed by means of a quartz prism the active rays are found to lie far beyond the limits of the visible region.

This method of showing the effect is not so direct as that used originally by Hertz, and for that reason is perhaps not so much to be commended. But when the experiments are to be shown to a large audience the gain in certainty and ease of operation is so great as to make up for this disadvantage.

The experiments of Wiedeman and Ebert[1] which immediately followed those of Hertz, brought out several points regarding the effect of ultra-violet light whose importance has perhaps not been fully recognized. Hertz's experiments showed that when a spark gap was just a little too long to allow sparks to pass, the illumination of the terminals by ultra-violet light would cause sparking to begin. It was not unreasonable to think that the effect of the ultra-violet rays was merely to overcome the slight opposition to the *beginning* of a discharge, and that when sparking had once begun it would continue without further outside help. The experiments of Wiedemann and Ebert showed, however, that the effect of illumination was to diminish the effective resistance of the spark gap whether a discharge was taking place or not. A convenient method of showing this effect is the following:

[1] Wiedemann's Annalen, Vol. 33, p. 240. See also *Science*, Vol. IV., No. 102, where an account is given of the work done in investigating the effect of light upon the discharge of electrified bodies up to the close of the year 1896.

A Geissler tube connected in series with the spark gap is caused to rotate by any of the numerous devices used for this purpose. A little motor, which will keep the speed of rotation approximately constant without attention on the part of the lecturer, is to be recommended. If sparks from a Holtz machine are caused to pass at the spark gap, the rotating Geissler tube will show each discharge as a sharply defined spoke in a luminous wheel. The effect of illuminating the spark gap is now to increase the frequency of sparking. Since the effective resistance offered by the spark gap has been diminished, a discharge will take place at a lower difference of potential than before. In order to build up this smaller potential difference a shorter time is required; so that the sparks will follow each other much more rapidly when the spark gap is illuminated than when it is kept in the dark. This is shown in the Geissler tube by the fact that the spokes in the luminous wheel become much more numerous. The change is a very striking one, and with well polished terminals at the spark gap is perfectly definite and certain. So long as no ultra-violet light falls upon the gap the rotating Geissler tube shows a luminous wheel with rather widely separated spokes. On removing a screen so as to allow rays from an arc light to fall upon the gap, the number of spokes will be increased two or three times. In some cases, where the adjustment is just right, the number of spokes will be increased so greatly that the discharge through the tube appears to be continuous, as though the tube were connected directly with the machine. The change in the appearance of the Geissler tube is accompanied by a change in the sound produced by the sparks. But this latter effect can hardly be observed at a distance.

The experiments described above, while extremely easy and certain, require careful attention to some essential points. In the first place the spark gap must be short. The best length seems to be between 0.5 mm. and 1 mm. For a length of spark greater than 2 mm. I have been unable to detect any influence at all due to illumination. The reason for this is not far to seek when we remember that the effect of the ultra-violet rays is merely to facilitate the discharge of *negative* electricity. No action is produced either at the positive terminal or in the space between the terminals. The effect of illumination seems to be equivalent to reducing the length of the air gap by a certain constant amount. When the spark is short the change in the effective resistance will therefore be a large fraction of the whole resistance; while if the spark is long the effect of illumination will be relatively insignificant.

As terminals for the spark gap, I have found brass knobs about half a centimeter in diameter to be most satisfactory. They should be polished frequently, even when there is no sign of deterioration on the surface; merely to rub the surface for a few moments with fine emery paper and

oil will sometimes cause the experiment to work successfully when all other adjustments have failed. In the second experiment described above it is necessary to use a Holtz machine rather than an induction coil. In fact I have found the Holtz machine in all cases more convenient for these experiments.

The distance between the spark gap and the arc should not exceed a meter for perfectly certain results, although some effect may be obtained at a greater distance under favorable conditions. It is perhaps unnecessary to add that glass lenses can not be used in connection with the arc. A thin sheet of glass forms just as effective a screen as a sheet of metal.

The Electrostatic Capacity of a Two-Wire Cable.

By Geo. W. Patterson, Jr.[1]

My reason for preparing this paper was the fact that I had found no treatise or text-book on electricity which gave an expression for the electrostatic capacity of a two-wire cable. Since preparing it I have found in Webster's recent book "The Theory of Electricity and Magnetism" an expression for the capacity of excentric cylinders from which my final formula might be deduced by one who is familiar with transformations from electrostatic to practical electromagnetic units. Something analogous is given by Heaviside in his electrical papers, but it is not quite to the point which I sought.

The electrostatic capacity of cables is a matter of great importance in telephony. The distance over which the telephone can be used, other

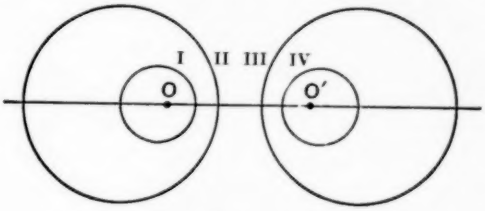

Fig. 1.

things being the same, is dependent practically on the product of resistance and capacity. For this reason it is desirable that the practical engineer should understand the effect of dimensions and materials in determining the capacity of a particular cable. I shall briefly derive the expression for the capacity of two concentric cylindrical conductors and from that pass on to that of two equal and parallel cylindrical conductors.

[1] Read at the Detroit Meeting of the A.A.A.S., August, 1897.

Let us consider a system of coaxial cyclinders a, b,. . .n, of indefinite length, and ranging in size from a of small radius r_a, to n of large radius r_n. Suppose a to be a conductor charged with m units of electricity per unit of length. The surface of each cylinder is an equipotential surface of the system. To determine the electric intensity (force on unit charge) at any point distant r from the axis, a well known property of equipotential surfaces may be used. The equipotential surface passing through the point may be supposed to be replaced by a conducting surface in communication with the inner conductor. The electric charge will spread over this surface, giving it a surface density σ equal to $m \div 2\pi r$. The electric intensity at the point is $4\pi\sigma \div K = 2m \div rK$, where K is the dielectric constant of the medium. The difference of potential V between any two equipotential surfaces is the line integral between them of the electric intensity. This line integral, while the same for any such line, is most easily computed along a radius of the system. Its value is

$$\int_a^n \frac{2mdr}{Kr} = \frac{2m}{K} \text{ Nap. log. } \frac{r_n}{r_a} = V \qquad (1)$$

and the capacity of C per unit of length of a condenser having a and n for its plates, is

$$C = \frac{m}{V} = \frac{K}{2 \text{ Nap. log. } \frac{r_n}{r_a}} \qquad (2)$$

This result is in *C. G. S.* electrostatic units. It is more convenient to use common logarithms and the practical electro-magnetic unit, the microfarad. For which purpose (2) must be multiplied by the modulus of the common logarithms and be divided by the square of the velocity of light, and allowance made for the microfarad being 10^{-15} of the absolute unit.

$$\text{Then } C = \frac{0.4343\,K}{18 \times 10^5 \log. \frac{r_n}{r_a}} = \frac{2413 \times 10^{-10} K}{\log. \frac{r_n}{r_a}} \qquad (3)$$

Passing now to the case of two equal parallel cylindrical conductors of very small radius, at O and O' of the figure, charged with equal and opposite charges $+ m$ and $- m$ per unit of length, it may be seen by a similar course of reasoning that the difference of potential V between D, a point midway between them, and any point distant r' from the axis of the first and r'' from the axis of the second cylinder, is the sum of two expressions similar to (1):

$$\frac{2m}{K} \text{ Nap. log. } \frac{r_d}{r'} \text{ and } \frac{-2m}{K} \text{ Nap. log. } \frac{r_d}{r''}$$

where r_d is the distance from the axis of each to the point D. This sum is

$$\frac{2m}{K} \text{ Nap. log. } \frac{r''}{r'}$$

The locus of all the points when the ratio $\frac{r''}{r'}$ is constant, is a third cylinder, whose axis lies in the plane of the axes of the first and second, and further away from D. *I*, *II*, *III* and *IV* are traces of four cylinders corresponding to different values of the ratio $\frac{r''}{r'}$. Each of these cylinders is an equpotential surface of the system and its radius is a mean proportional between the distances from its centre to the lines O and O'. Suppose now that cylinders *II* and *III* are parallel conductors of equal radii R, surrounded by a dielectric whose constant is K. Let r be the smallest value of r' and let d be the least thickness of the dielectric between them. By geometry,

$$r = \frac{1}{2}\sqrt{4Rd + d^2} - \frac{d}{2}.$$

The corresponding value of

$$r'' \text{ is } \frac{1}{2}\sqrt{4Rd + d^2} + \frac{d}{2}.$$

The difference of potential V, between the point D and the surfaces of *II* and *III* is

$$\frac{2m}{K} \text{ Nap. log. } \frac{r''}{r'}$$

and that between *II* and *III* is double this:

$$\frac{4m}{K} \text{ Nap. log. } \frac{r''}{r'} = \frac{4m}{K} \text{ Nap. log. } \frac{\sqrt{4Rd + d^2} + d}{\sqrt{4Rd + d^2} - d}$$

The capacity C per unit of length of a condenser having *II* and *III* as its plates, is

$$4 \text{ Nap. log. } \frac{K}{\frac{\sqrt{4Rd + d^2} + d}{\sqrt{4Rd + d^2} - d}}$$

in electrostatic units. Changing to microfarads and common logarithms this becomes:

$$\log. \frac{1206 \times 10^{-10} K}{\frac{\sqrt{4Rd + d^2} + d}{\sqrt{4Rd + d^2} - d}}$$

microfarads per centimeter length, or,

$$\log. \frac{\frac{1206 \times 10^{-5} K}{\sqrt{4Rd + d^2} + d}}{\sqrt{4Rd + d^2} - d}$$

microfarads per kilometer.

In practice the two conductors are twisted together, and the dielectric about them, moreover, is not of indefinite extent, which causes slight, but negligible changes in the formula.

EXAMPLE.—Suppose the two wires to have diameters equal to the distance between them, then the capacity in microfarads per kilometer will be

$$\frac{1206 \times 10^{-5} K}{\log. \frac{\sqrt{5} + 1}{\sqrt{5} - 1}} = 0.0282\ K,$$

or per mile $0.0454\ K$. For air insulation K is 1. For paraffine it is about 2, and for rubber about 2.25. In some telephone cables, using air and paper strips as insulation, a value of 0.05 microfarads per mile has been obtained with the above dimensions.

In case the two conductors are of unequal radii the calculation becomes somewhat more complicated, but as the case is of relatively little importance it will be sufficient if the method of procedure is merely indicated. Let us take as the conductors whose electrostatic capacity is sought, cylinders *I* and *III* of the figure. Call their radii R_1 and R_3 and let D be the least thickness of the dielectric between them. Let r_1 be the distance, along the line of centres, of the image O of cylinder *I* from its surface, and r_3 the corresponding quantity for cylinder *III*. The difference of potential between *I* and *III* for unit positive and negative charges respectively per unit of length is

$$V = \frac{2}{K} \text{Nap. log.} \frac{(D + r_1)(D + r_3)}{r_1 r_3}$$

and the capacity of the system per unit of length is,

$$C = \frac{K}{2 \text{ Nap. log.} \frac{(D + r_1)(D + r_3)}{r_1 r_3}}$$

In practical units and common logarithms this becomes

$$C = \frac{2412 \times 10^{-5} K}{\log. \frac{(D + r_1)(D + r_3)}{r_1 r_3}}$$

microfarads per kilometer.

As O and O' are inverse points with reference to each cylinder we have the relations

$$r_1 = \frac{R_1 (D + r_3)}{D + R_1 + r_3} \text{ and}$$

$$r_3 = \frac{R_3 (D + r_1)}{D + R_3 + r_1}$$

From these equations the values of r_1 and r_3 may be determined as functions of R_1, R_3 and D; and by substitution in the previous equation, the value of C may be determined. The resulting expression is quite long and complicated when expressed in functions of R_1, R_3 and D; but in any particular case, when definite numerical values are assumed, the calculation presents no difficulties.

NEW BOOKS.

The Phase Rule. By WILDER D. BANCROFT. 8vo, pp. 255. The Journal of Physical Chemistry, Ithaca, N. Y., 1897.

In the preface the author suggests as natural divisions of physical chemistry, Qualitative Equilibrium, Quantitative Equilibrium, Electrochemistry, and Mathematical Theory. This classification seems arbitrary and the wisdom of presenting a subject so quantitative in its nature and development with as little mention as possible of the quantitative side, is questionable; yet, by this very method of treatment much is gained in the way of simplicity and singleness of point of view.

The Gibb's Phase Rule furnishes the basis of classification, systems of one, two, three and four components being taken up in order. The simpler cases, starting with water in its three phases, solid, liquid and vapor, thus receive attention first, and the advance to the more complicated systems is gradual.

The work has been well planned and well executed. We are given a very complete presentation and a full critical discussion of the phenomena involved. Much of the experimental matter has been published before except in the original articles, the references to which are a valuable feature of this treatise.

Some of the views advanced are subject to criticism; for example, the distinction between solvent and solute in the liquid phase of two components, and some of the criticisms of the work of other authors. But the author is in general careful to distinguish between theories well established by experiment, and those that have no such support.

The book is a valuable addition to the literature of physical chemistry and should be read by all who are engaged in this line of work. The suggestion of many interesting problems, which have not yet received experimental attention, should serve as a stimulus to much fruitful investigation.

CLARENCE MCC. GORDON.

Practical Electrical Measurements. By E. H. CRAPPER. 8vo, pp. 125. London, Whittaker & Co., 1897.

This small laboratory manual is intended for first year students in English technical schools and it will serve its purpose very well. While perhaps too large a part of the book is taken up by the study of the action of a magnet upon another, of the tangent galvanometer and the numer-

ous exercises connected with the latter, there are many points which will recommend the use of the book. So the experiments in the chapter on "Electromagnetism," showing the nature of the magnetic field set up by a current, are excellent and stand in a striking contrast to those given in our corresponding American books, in which too little stress is laid upon the correct conception of the magnetic field. The platting of the lines of magnetic force around a straight wire and a solenoid, carrying a current, should lead up to similar experiments with magnets rather than the reverse, and they will explain the modern idea of poles, magnetization, etc., a great deal better than the usual elementary exercises given under magnetism. Work of this kind may be carried even farther. For instance, a model representing a D'Arsonval galvanometer is easily constructed and shows in a very clear manner the action of the instrument.

Another good feature of Mr. Crapper's book are the exercises given as an appendix to many of the practical experiments. They will teach the student to think, deepen his interest in the experiment and draw his attention to the accuracy obtainable by the method he has just applied.

The number of specific electrical experiments seems rather small in comparison with that devoted to magnetism and no work with the condenser is given, but it may be that this is reserved for the second volume, dealing with "Practical Electrical Engineering," a book which the author has in preparation as a sequel to the present one.

K. E. GUTHE.

The Localization of Faults in Electric Light Mains. By F. CHARLES RAPHAEL. Octavo. pp. 184. New York, The D. Van Nostrand Company. London, The Electrician Publishing Company, 1896.

The author of this useful book states that although the localization of faults in telegraph cables has been dealt with fully in several handbooks, the treatment of faulty electric light and power cables has never been discussed in an equally comprehensive manner. The methods of treatment of the two cases necessarily differ, inasmuch as faults in telegraph cables are rarely localized before their resistance has become low as compared with the resistance of the cable itself, while the contrary is more usually the case in electric light mains. The book is confined to this one subject and thus differs from the more general works on electrical testing. The author has given an adequate and rather complete treatment of his subject. The book is a practical one, intended as a handbook for central station engineers; yet it has brought together so much that is instructive that it may well be given a place as a student's reference book in electrical laboratories.

The author divides "faults" into three classes: (1) Discontinuity of

the conductor; (2) Short circuits; (3) Electrical contact between the conductor and the earth. The major portion of the book deals with faults of the third class.

The theory of the methods employed and the mathematical work required therefor is omitted from the body of the book and is incorporated in a concluding chapter.

The opening chapter is introductory and descriptive of galvanometers, switches and various testing instruments employed. Chapters II. and III. pertain to insulation testing during working of low and high pressure mains, respectively, a subject quite germane to that of localizing faults. The treatment of the insulation testing of low pressure mains is complete, but we find the few pages on testing of high pressure mains rather inadequate. The main portion of the book is divided into chapters on "General Methods of Fault Localizing," "Localizing during Working" and "Fault Signalling Networks."

The author has performed a service in collecting together material hitherto widely scattered and placing it in systematic order. The author has added some new material, *e. g.*, a discussion of how low must the resistance of a fault be to be localized to a given degree of accuracy. As a result of this investigation, he determines a formula for the possible error (in yards) in localizing a fault under given conditions.

The concluding chapter on "Algebra," which contains all the mathematical work of the book, although good as far as it goes, is not complete. Unfortunately, too, the author does not refer in the body of the book to the proofs given in this concluding chapter. The incompleteness of the theoretical treatment weakens the book in its use as other than a practical manual. The valuable references which the author has given to original sources might be more complete.

The typography and illustrations are only fair. Many of the diagrams have been prepared especially for the book; the illustrations of apparatus are taken chiefly from trade catalogues.

FREDERICK BEDELL.

An Introductory Course in Quantitative Chemical Analysis. By PERCY NORTON EVANS. 12mo, pp. iv + 81. Ginn & Company, Boston, 1897. (*Received.*)

The Commercial Uses of Coal Gas. By THOS. FLETCHER. Pp. 104. Fletcher, Russell & Co., Warrington, Manchester and London. 1897. (*Received.*)

Elements of Geometry. By ANDREW W. PHILLIPS and IRVING FISHER. viii and 540 pages. Harper & Brothers, New York, 1897. (*Received.*)

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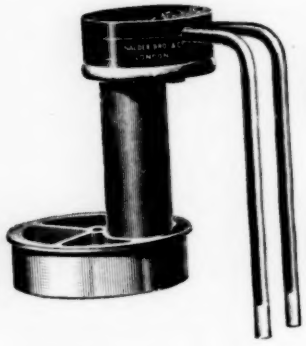

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